



Essays in Finance and Innovation

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Essays in Finance and Innovation

Abstract

This dissertation consists of three essays on Finance and Innovation.

The first essay argues that openness of research inputs can enable greater exploration of new research lines, particularly by academic researchers. We test this hypothesis by examining a natural experiment: NIH agreements that reduced the access costs imposed on academics regarding certain genetically engineered mice. We find that increased openness encourages the exploration of more diverse research paths, and does not reduce research into the creation of new genetically engineered mice. Overall, our findings highlight a neglected cost of strong intellectual property restrictions: lower levels of exploration leading to a reduction in the diversity of research output.

The second essay analyzes clustering in firms' layoff behavior and its links to financial markets. We develop a model in which managers delay layoffs during good economic states to avoid damaging their reputation. We test the model's predictions by comparing the layoff behavior of publicly-listed and privately-held firms, and find that the layoffs of public firms are twice as sensitive to recessions. In addition, we find that the firms which cluster layoff announcements at high frequencies are also more likely to engage in mass layoffs during recessions. Our findings suggest that reputation management is an important driver of layoff policies both at daily frequencies and over the business cycle, and can have

significant macroeconomic consequences.

The third essay explores the relationship between funding availability and innovative output. I develop a theory of credit constraints in multi-stage research, and predict that greater funding leads to both transitions into the private sector and a shift toward shorter-horizon projects. Analyzing the patent output of a panel of life-science researchers linked to top universities, I find that greater funding availability leads to an increase in transitions from academia to the private sector and a higher quantity of patent output, but a decrease research value, innovative scope, and the time horizon of subsequent applications. These results indicate that profit-motivated funding not only increases the quantity of innovation, but also leads to a significant shift in the type of projects being pursued.

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Chapter 1

Of Mice and Academics: Examining the Effect of Openness on Innovation

1.1 Introduction

Over the past three decades, there has been a significant increase in the scope of formal intellectual property (IP) rights, such as patents, over scientific knowledge traditionally maintained in the public domain (Mowery, et al, 2001; Murray, 2002; Heller, 2008). This dramatic expansion of IP rights for early-stage research tools has spurred a wide-ranging policy debate, with particular attention paid to the impact that IP plays in shaping both fundamental scientific advances and the incentives for follow-on research. (Merges and Nelson, 1990; Gallini and Scotchmer, 2002).

The impact of intellectual property rights on the rate and direction of scientific research is subtle. When strong and broad IP rights are available in a sequential innovation setting, early-stage researchers can capture the value of their innovation by imposing access fees, thus enhancing the incentives for early-stage research but imposing a tax on follow-on research activities (Scotchmer 1996; 2004). In its simplest form, this perspective focuses attention on the trade-off between enhancing incentives for development along existing research lines (achieved by relaxing IP protection), and providing incentives for the creation of new upstream research tools (achieved through tighter IP protection).

While this trade-off is important, it neglects the distinctive nature of cumulative dis-

covery within the academic research enterprise. Building on an emerging body of research in the new economics of science (Dasgupta and David, 1994; Stern, 2004), Aghion, Dewatripont and Stein (2008, henceforth ADS) emphasize the role of intellectual freedom - granting control rights to researchers to select their own research agenda. A control rights approach focuses attention on the role of exploration: scientific discoveries are both sequential and multi-purpose, and new follow-on research paths can be discovered when scientists freely explore the potential applications of existing research. In an “open” research environment, researchers have low-cost and independent access to prior discoveries and research tools. Such an environment encourages not only direct (perhaps commercially oriented) exploitation by follow-on researchers, but also the exploration of new research lines and entry by researchers who are new to an emerging research area. Intellectual property rights that impose significant transaction costs or impede access to particular discoveries are likely to limit the incentives and the productivity of these more exploratory efforts.

The main prediction analyzed in this paper is that in a research setting characterized by a high level of intellectual freedom, openness will not simply increase the level of research output, but will be closely associated with more diverse and exploratory research. We evaluate this idea in a very distinctive research setting: the invention, development and application of genetically engineered (transgenic) mice by academic scientists (mainly biologists) in the period from 1980 to 2010. While mice might seem to be a niche research tool, the invention of methods to precisely engineer mice to exhibit particular characteristics (e.g., to be predisposed to a specific disease) enabled scientists to dramatically expand their ability to explore the biological basis of disease or evaluate the impact of different drugs. Scientists developed three complementary but distinctive methods for mouse engineering - known as the Onco, Cre-lox and Knock-out technologies. As emphasized by a leading researcher, “...at the end of 1980, in a period of a few months, an entirely new era in mouse genetics began, with the creation of the first transgenic mice, initiated by the abrupt and then continuing entry of molecular biological techniques into what had, until then, been a classical genetic system. What ensued was an explosion of knowledge when a myriad of new biological and molecular insights appeared over the following years” (Paigen 2003 quoted in Murray 2010). The new tools were used to develop a wide range of specialized research mice, and both the researchers who developed the mice as well as those who could

access the mice from colleagues or centralized repositories were able to undertake a wide range of both basic and applied follow-on research. It was no surprise that the developers of one of the methods - the Knock-out technology - ultimately received the Nobel Prize in 2007.

Within this setting, we evaluate the idea that a shift to a higher level of openness (e.g., by making particular mouse engineering technologies more accessible to follow-on researchers) will not simply increase the level of follow-on research but will also result in more diverse and exploratory research. We exploit natural experiment from the late 1990s that impacted the degree of openness faced by researchers who wanted to access or make genetically engineered research mice using two of the three genetically engineered mice technologies mentioned above. In particular, during most of the 1990s, DuPont enforced broad patents over two of the technologies (Onco and Cre-Lox). DuPont established an aggressive approach towards licensing: researchers who wanted to use these mice faced stringent restrictions, such as pre-publication article review by Dupont and complicated reach-through rights that would impinge on any subsequent commercial innovations.¹

In the late 1990s, the National Institutes of Health negotiated two Memoranda of Understanding with DuPont that granted academic researchers low-cost, royalty-free and independent access to both the use of DuPont's methods and the transgenic mice created with them. These agreements created a simple and standardized one-page contract for gaining access to the mice, and facilitated their availability through the Jackson Laboratory, the world's largest non-profit research mice repository. This unanticipated agreement between the NIH and DuPont constituted a clear openness shock for the mouse genetics research community: the research tools covered by the patents - hundreds of varieties of Cre-lox and Onco mice developed in the early 1990s - shifted from a regime of high access costs to a regime where mice were made readily available (at essentially marginal cost) to the academic research community.

Our empirical approach takes advantage of several key aspects of these NIH agreements and the nature of mouse genetics research to develop a differences-in-differences estimate

¹Indeed, the broad nature of these patents, and DuPont's enforcement approach, were one of the key motivating examples for the influential analysis of Merges and Nelson (1990), which directed attention to the crucial role of patent scope.

of the impact of increased openness on both the level and nature of follow-on research. First, each genetically engineered mouse is associated with a journal article that describes its initial development; we refer to these linked publications as “mouse-articles.” We are able to construct a treatment sample based on mouse-articles affected by the NIH agreements, and a control sample of mouse-articles unaffected by the agreements (composed of mice that were engineered using two alternative technologies - “Knock-out” or “Spontaneous”). Second, the precise timing and scope of the NIH agreements were unanticipated by the mouse genetics community. In effect, there was a sudden and permanent reduction in access costs associated with the mice in our treatment sample, with no change in access costs for our control sample. Finally, we take advantage of detailed bibliometric data for follow-on citations to the mouse-articles to characterize how the openness shock changed the nature of subsequent research along a number of important margins.

In implementing our empirical analysis, we study the citations to a sample of more than 2000 mouse-articles, approximately 10% of which are associated with the Cre-lox and Onco technologies and so were impacted by the shift in openness that resulted from the NIH agreements. By comparing citations to the mouse-articles before and after the agreement (and comparing them to the evolution of citations in the control sample), we are able to isolate the causal impact of a shift in access costs on the level and nature of research. In addition to examining whether there is a net increase or decrease in the level of citations, the bulk of our analysis examines how the composition of citations differs after the openness shock. Specifically, we construct measures capturing whether the research community using a particular mouse in any given year is composed of new authors joining the community (e.g. the number of new authors citing the mouse-article), whether a particular mouse is generating new and previously unexplored lines of research (e.g. whether the citations include keywords that had never before been linked to a particular mouse-article), and whether that follow-on research is published in journals that are linked to more basic or more applied research. Finally, we directly examine the impact of the NIH agreements on the creation of new mouse-articles. While the reduction in access costs paid to early-stage researchers would normally decrease the incentives to create new mice and publish their associated mouse-articles, a setting where free exploration is central to the production of scientific knowledge would be consistent with a positive or neutral effect of increased

openness on mouse creation.

Our results are striking. The NIH agreements are not simply associated with an increase in the level of follow-on research. We find that the bulk of these increased citations are associated with research produced by “new” researchers and institutions. Specifically, the boost in citations to a given mouse-article in the post-NIH-agreement period comes from researchers that had not cited that mouse-article prior to the NIH agreement. In addition, the NIH agreements resulted in a significant increase in the diversity of follow-on research: there is a decisive increase in the diversity of the journals in which mouse-articles in the treatment group are cited, and, in the number of previously unused “key words” describing the contributions of the citing research. Intriguingly, our data suggest that the NIH agreements are not associated with a reduction in the creation of new mouse-articles (i.e. the use of the two methods to develop new mice); instead, the development of new genetically engineered mice either increased or remained the same after the agreements. Taken together, these findings are consistent with the view that exploration is a central component of academic innovation.

It is important to consider whether our results are simply reflecting the classical tension between the provision of incentives for a first-generation innovation versus the provision of incentives for downstream researchers. Our qualitative examination of this scientific community suggest that the changing IP rights had little effect on the production of novel methods to engineer mice themselves: from the start of this revolution in molecular biology, scientists involved in developing new techniques were driven more by their potential to develop powerful new tools and enable diverse follow-on research than by the direct rewards of intellectual property. Further, in this case, patentability itself was a surprise - at the time of the development of the three mouse engineering technologies, the researchers did not anticipate that mice were potentially patentable. Indeed, the patent that was ultimately granted on the Onco-Mouse was the first patent granted on a genetically engineered mammal. In light of this, we suggest that the classical tension between early- and late-stage research incentives does not capture the full range of factors which determine the outputs of academic innovation, and propose that openness and exploration are also primary drivers of the research process.

The paper is organized as follows. Section 2 motivates the analysis by elaborating

on the effects of openness on scientific knowledge production. Section 3 describes the experiment we use to explore the effects of increased openness on the level and composition of research flows. Section 4 outlines our identification strategy. Section 5 presents the data and summary statistics. Section 6 presents the empirical results, and Section 7 concludes.

1.2 The impact of openness on scientific knowledge production

The primary focus of this paper is on how the degree of openness associated with scientific research tools impacts the level and nature of research using those tools. Our analysis begins with the framework of Green and Scotchmer (1995), who view early-stage research as providing a set of tools which serve as inputs to later-stage work. Under a regime of strict IP rights, upstream tools developers are tool-specific monopolists, and so are able to impose significant access charges when a specific research tool is required for a follow-on research project. The natural tradeoff in this environment is between providing incentives for tool creation through strong and long-lasting IP rights, and facilitating later-stage development by relaxing IP rights and providing low-cost access to existing tools. As long as IP rights holders cannot engage in perfect price discrimination with potential follow-on researchers (e.g., because of asymmetric information), a regime of strict IP rights for upstream tools will be associated with higher prices and lower quantities relative to the social optimum. Thus, if one focuses on a linear model of the innovation process, increased openness will (in equilibrium) decrease the rate of tool creation and increase the level of follow-on research.²

However, as emphasized in ADS (2008), this linear view of innovation neglects two fundamental aspects of the scientific research process. First, a single basic research discovery may have multiple follow-on applications, and there may be diversity among these follow-on research paths in terms of whether they are more exploratory or more closely linked to near-term market application. Second, the salience of alternative research paths

²This hypothesis has found support in empirical studies such as Furman and Stern (2011) and Williams (2011).

will be greatly enhanced in an academic research environment, where researchers are granted control rights to pursue their own scientific research agenda (“academic freedom”) in exchange for lower wages (Stern, 2004; ADS, 2008). As shown in Appendix A, openness plays a subtle but central role in the context of a control rights model such as ADS in which “free” researchers can pursue multiple research paths.³ First, openness directly lowers the cost of accessing the ideas of others, allowing free researchers to improve upon them when the original inventor lacks to expertise or desire to do so. This enables more researchers to contribute to the development of a given field, and allows for a more diverse range of ideas to be pursued in later-stage research. Openness therefore increases the range of potential applications and the number of contributing researchers.

Second, within a control rights framework, the earliest stages of any sufficiently long research line are optimally performed in academia (ADS, 2008). Unlike their industrial counterparts, academic researchers are more likely to be credit-constrained, and their research outputs are more difficult to transfer using reach-through contracts. Consequently, high costs of access will serve as a particularly strong impediment to research lines that are too long to be profitably pursued entirely within an industry research environment. Increased openness is therefore likely to have a greater impact on the earliest stages of long-horizon, speculative research lines.

Third, openness will be particularly important when there are “multiple tolls” - if a research line has multiple stages until completion, and an input has a high access cost, the cost of accessing that research input may be incurred multiple times as the line progresses. Once researchers are locked in to using a specific research tool, they are open to hold-up at every subsequent stage. This is most salient when the different stages of a research line are likely to be undertaken by distinct researchers (as would occur in academia). This effect is exacerbated for more exploratory research lines that are characterized by a branching structure where value is dispersed across a range of newly-generated research directions. When a single discovery induces multiple follow-on research paths, a lack of openness will be most detrimental to the most speculative paths that involve the highest number of

³See Appendix A for a formal model of openness which builds on the ADS control rights approach to academic innovation. Merton (1973), Dasgupta and David (1994) and David (2003) offer rich and comprehensive discussions of the role of openness and freedom in the system of Open Science.

potential follow-on stages.

Finally, because increased openness facilitates exploration, and therefore the utilization of existing innovations, it can also generate an expanded set of applications for IP rights holders. While the lower access costs might decrease the IP holder's revenue in the short-run, the long-run expansion of possible (commercial) applications works to counteract this decline. This countervailing effect is strongest in an academic setting where the potential for enhanced diversity of applications is strongest. In contrast to the predictions from the linear model, a policy of increased openness need not reduce the rate of innovation by early-stage researchers.⁴

In contrast with a linear model, a focus on multi-stage exploratory innovation suggests that an increase in openness will have an impact which goes beyond a temporary one-off effect. Its initial impact is an increase in research which advances existing lines to the next stage of their progression, but this effect will persist because most research lines, particularly in academia, are many stages from completion. Further, because the development and exploration of existing lines can lead to new research directions, there is a positive probability of a long-term flow of new research lines which continue long after the original lines have ended.

As we move to our predictions, it is useful to state the relevant null hypothesis. Under the linear model, openness is expected to result in a proportionate increase in all different types of follow-on research activities. Since all types of follow-on research are equally likely to depend on a given research tool, then all types of follow-on research will experience the same reduction in cost after an openness shock. Consequently, the linear model implies that there would be no systematic differences in the impact of openness across different types of follow-on research uses. In other words, our null hypothesis is that the impact along the intensive margin (the output of existing researchers) is equal to the impact along the extensive margin (the inflow of new researchers) in response to a change in

⁴Bessen and Maskin (2009) highlight a closely-related effect in the context of the software industry: in a setting where innovations are complementary, openness reduces direct payments to an existing innovation, but increases the likelihood of follow-on innovations, thus increasing expected future profits. Of course, our analysis does not provide a thorough evaluation of the question of how to finance and/or reward the discovery of the initial research input. This could happen either through publicly subsidized research or through public buy-outs of (private) patents as in Kremer (1998).

openness of an early-stage technology.

We can now turn to the specific hypotheses we will test in our empirical analysis. First, there is the traditional trade-off effect: an increase in the level of openness of existing research reduces the costs associated with its use along a given research line. Second, particularly since we are examining the impact of openness in the context of academic innovation, reducing the costs of accessing key research inputs should widen the potential pool of researchers and institutions conducting follow-on research on any given research idea. Third, openness makes researchers with high levels of freedom (academics) more likely to engage in speculative exploration that broadens the diversity of research lines being pursued, and this effect is likely to be both long-term and increasing in the years immediately after the change in openness. Fourth, based on the nature of access costs and the level of familiarity that follow-on researchers have with the existing technology, we predict a differential effect on basic relative to applied follow-on research.⁵ Finally, enhanced openness expands the range of potential follow-on research applications (which could not have been fully anticipated by the original tools developer); thus, increased openness need not reduce the rate of innovation by early-stage researchers.

The remainder of this paper examines the above predictions in the context of a specific natural experiment: the change in openness resulting from NIH agreements covering genetically engineered mice, which took effect in the late 1990's. In the following section, we elaborate on the details of this empirical setting.

⁵We expect the impact on basic research to dominate when researchers face high access costs driven by significant difficulty in directly obtaining research inputs during the pre-openness period. This form of access costs would prevent them from gaining familiarity with the upstream idea that they seek to explore, leading to an initial emphasis on basic-science research following an increase in openness. By contrast, the increase in applied research will tend to dominate when the groundwork of basic understanding of the existing innovation has already been established. This is most likely to be the case when access costs are primarily composed of reach-through rights to follow-on research, and when these costs are high enough to deter applied-science research during the pre-openness period.

1.3 Empirical setting: experiments in the openness of genetically engineered mice

This section overviews our empirical context and, in particular, the natural experiments that significantly shifted the level of openness for two broad categories of genetically engineered mice.⁶ Mice play a central role in the study of cancer and other human diseases due to their genetic likeness to humans (the similarity between mouse and human genomes is 85% on average).⁷ Throughout the twentieth century, scientists in mouse genetics relied on spontaneous mutations for their disease studies: researchers bred mice that naturally exhibited particular disease-linked symptoms or behaviors.⁸ To facilitate their efforts, the research community developed open-access institutions, notably the Jackson Laboratory (a mouse repository in Bar Harbor, Maine) to classify, breed, and distribute specialized research mice to the academic community (Rader, 2004).

In the early 1980s, advances in molecular biology and in the ability to manipulate embryonic stem cells allowed researchers to develop a set of systematic and precise methods for engineering specialized mice as research tools, greatly expanding the application of research mice in life sciences research.⁹ Three breakthroughs were particularly important. First, in a discovery that was subsequently awarded the 2007 Nobel Prize in Medicine, Mario Capecchi of the University of Utah and his collaborators developed the Knock-out technology, enabling researchers to delete specific genes in research mice. Second, with partial funding from DuPont Corporation, Professor Phillip Leder at Harvard University

⁶Murray (2009) provides a detailed overview of the mouse genetics revolution and the role of intellectual property and openness within the mouse genetics research community. See also Rader (2004).

⁷See `http://www.ornl.gov/sci/techresources/Human_Genome/faq/compngen.shtml`

⁸Given the value of such mutations, researchers also developed techniques to significantly increase the rate of mutation of research mice, such as the exposing pregnant mice to high levels of radiation (Murray 2007 and 2009).

⁹These methods of mouse engineering are complex and costly. To create a mouse with particular genes inserted within a mouse genome, scientists must first inject foreign DNA into mouse eggs, transplant the eggs into female mice, and, if successful, monitor and breed the incorporation of the genes into the offspring. During our sample period, the development of a mouse line from scratch likely involved at least 18 months of laboratory research and a significant investment of time and attention by a principal investigator (Rader, 2004; Murray, 2009).

developed the “OncoMouse” method, which provided a means for inserting (rather than deleting) genes into an embryo, thereby making mice susceptible to particular forms of cancer and other diseases. Finally, researchers in the life sciences division of DuPont, including Brian Sauer, developed the Cre-lox technology - a precise cutting and pasting tool that turns off genes in specific tissues or organs. In practical terms, these advances allowed researchers to develop three new types of research mice: Knock-out, Cre-lox and Onco mice, which, along with the previously-available spontaneous mice, could serve as critical scientific research inputs.

1.3.1 Intellectual Property in the Mouse Genetics Revolution

The revolution in mouse genetics coincided with two important shifts in the role of IP rights in life sciences research. In 1980, the Supreme Court decision in *Diamond v Chakrabarty* established the patentability of genetically engineered organisms, and the Bayh-Dole Act affirmatively allowed universities to seek patents over federally-funded research.¹⁰ While many observers took universities’ growing patent portfolios as an indicator of their evolving role as engines of innovation (Henderson, Jaffe and Trajtenberg, 1998), some argued that strong IP rights could lead to rent-seeking and undermine research productivity (Heller and Eisenberg, 1998). This debate was particularly salient for researchers within the mouse genetics revolution. Each of the three main new technologies (Knock-out, Onco, and Cre-lox) received a relatively broad patent.¹¹ In the case of Knock-out mice, the University of Utah received a patent in 1987 but did not place strict IP restrictions on their use by follow-on researchers. Instead, Knock-out mice were made available at relatively low costs through the Jackson Laboratory.

Before proceeding we should note that for mouse genetics researchers, the potential to patent the novel mouse genetics methods (and thus control the mice produced with them) does not seem to be the key incentive in the development of any of the three technolo-

¹⁰These legal and policy shifts reflected, in part, increasing appreciation that certain types of academic research were increasingly dual in nature: fundamental scientific discoveries that could simultaneously have a high degree of commercial utility (Murray and Stern, 2007).

¹¹Knock-out mice were covered under U.S. Patent 4,687,737, Onco mice under U.S. Patent 4,736,866 and Cre-lox mice under U.S. Patent 4,959,317.

gies. First, most of the research was initiated prior to the Bayh-Dole Act and was therefore started in a period before academic researchers considered the patentability of their inventions. Second, while *Diamond v. Chakrabarty* upheld patents on genetically engineered organisms, none of the research scientists considered mice (mammalian organisms rather than *e. Coli*) to be patentable subject matter at the time they were conducting the original research. Third, all the scientists involved have described the powerful scientific incentives that motivated their research. They were in pursuit of new genetic engineering methods that could render the simple mouse a more powerful research tool, and make possible a wide range of important new experiments (Murray 2009). This was true for the development of the Knock-out technology by Capecchi, who had received partial funding from the Cystic Fibrosis Foundation as well as the NIH. It was also the case for Leder and his funders (DuPont), - as evinced by the lack of specific direction imposed in DuPont's funding arrangement and by Leder's lack of involvement in patenting decisions related to the Onco technology.¹² For the Cre-lox technology, Sauer initiated the work on its development while still at the National Cancer Institute.

In spite of the apparent lack of *ex ante* incentives provided by the possibility of patenting, the patents that were eventually granted over the Onco and Cre-lox technologies proved to be much more controversial than the patent over Knock-out mice (Merges, and Nelson, 1990). As a result of their partial funding of Harvard's Oncomouse discoveries and their internal development of Cre-lox technology, DuPont gained exclusive control over patents for these technologies. In contrast to the University of Utah, DuPont chose to exercise strict control over the distribution and use of mice that exploited the techniques covered by their patent portfolio. During the early 1990s, researchers (and their institutions) were obliged to obtain a license from DuPont when they sought to use an Onco or Cre-lox mouse. The detailed licensing agreement required annual disclosures to DuPont regarding experimental progress, limits on informal mouse exchange among academic researchers, and reach-through rights allowing DuPont to automatically receive licensing revenue from any commercial applications developed using either Cre-lox or Onco technology.

These requirements - amounting to very high access costs for follow-on researchers -

¹²See Murray (2009) for a more thorough analysis of these issues.

caused widespread discontent within the academic community.¹³ There were a number of attempts to subvert or blunt the impact of the DuPont licensing regime: notably, in 1992 Dr. Ken Paigan, then-director of the Jackson Laboratory, announced he would make Onco mice openly available without a license, directly contravening DuPont's IP rights. While some researchers took advantage of informal sharing or chose to access Onco mice from the Jackson Laboratory (opening themselves to a potential infringement suit by DuPont), most researchers (and their institutions) were wary of the legal repercussions that could arise from using these mice. In the case of Cre-lox mice, prior to 1998, researchers had no means of access through the Jackson Laboratory or any other open-access depository: DuPont maintained a near-monopoly on their distribution.

Thus, by the mid 1990s, researchers seeking to use a particular genetically-modified mouse faced one of several access-cost regimes. If the follow-on research required a Spontaneous and Knock-out mouse, it would generally be directly available from the Jackson Laboratory or another depository at relatively low cost.¹⁴ If the research required an Onco mouse, the mouse might be available informally through the researcher's peer-to-peer network or through the Jackson Laboratory. However, to use such a mouse was in direct contravention of DuPont's licensing requirements, and the risk of litigation served to increase the effective cost of access for follow-on researchers. If a Cre-lox mouse was needed, it might be available through informal exchanges among colleagues, but these too were beset by high access costs: Cre-lox developers invested considerable time and resources in the creation of the mouse, and often required co-authorship (or other types of non-monetary payment) in exchange for access. In addition, the exchange of such mice took place in the shadow of potential infringement suits by DuPont, as well as contravening the official policy rules of most universities.¹⁵ It was of course possible to access Cre-lox and Onco

¹³DuPont's practices were seen as "an enormous obstacle to free and open distribution of information and materials... it was a whole new way of doing science... it really affected the way the mouse research community works." (Murray, 2009).

¹⁴In addition to the unenforced Utah patent on knock-out technology, a small number of additional patents were granted over specialized knock-out mice. However, the intellectual property restrictions associated with these mice seem to have been negligible; further, their openness was not directly influenced by the NIH agreements we exploit in our empirical work.

¹⁵As described in Murray (2009), "The mice were fragile, and breeding lines had not been stabilized. This

mice by signing DuPont's licensing agreement. However, relatively few institutions or researchers did so prior to the NIH agreements of the late 1990's. Finally, it was possible for research teams to develop a new mouse within their laboratory as part of the research process. This approach could delay a project by at least 18 to 24 months, require significant resources (e.g., a full-time post-doc), involve investment in specialized mouse engineering skills, and, in any case, did not eliminate the risk of litigation based on infringement of the DuPont patent portfolio.

1.3.2 The openness shocks to Cre-lox and Onco mice

The degree of openness associated with Cre-lox and Onco mice shifted dramatically following their respective NIH agreements in 1998 and 1999.¹⁶ In the wake of a nearly decade-long campaign of pressure from the academic community, NIH Director and Nobel Laureate Harold Varmus successfully negotiated two "Memoranda of Understanding" among DuPont, the Jackson Laboratory, and the NIH. These agreements greatly decreased the access costs of the genetically engineered mice they covered for academic researchers. The Cre-lox agreement, announced in July of 1998, allowed the Jackson Laboratory and universities to distribute and share Cre-lox mice with a simple licensing process: a standardized one-page material transfer agreement and an institution-wide license.¹⁷ In addition, the Jackson Laboratory announced its commitment to acquire, breed, and distribute Cre-lox mice on an open-access basis. A similar agreement for Onco mice was reached one year later, in July of 1999, though the impact of this agreement was somewhat less

made it difficult to share in large numbers. One scientist who had managed to make Onco mice in the late 1980s recalled: 'I had a few requests for mice and offers of co-authorship. But I did not send them the mice... I was having to slow my own work down because they were breeding very poorly and so it was impossible to ship them around.' "

¹⁶We use the word "open" in the sense of widely accessible with clear and limited restrictions, much as it is used in the world of open-source software. In fact, the NIH agreements specified both a renouncing of the right to sue, and of reach-through rights to later work. While these agreements were imposed through a negotiating process, it should be noted that the outcome was strictly voluntary. While some open-source projects are created unilaterally and without external pressure, this is by no means a universal characteristic.

¹⁷While the actual NIH agreements pertained only to those with NIH funding, in reality this meant that virtually all academic researchers had direct access to the mice. However, the agreements had no effect on collaborative projects between academic and industry researchers, and on purely industry-run projects.

dramatic as the Jackson Laboratory had already been distributing Onco mice to researchers since 1992, albeit in violation of DuPont's IP requirements.

Over a two-year period, life sciences researchers experienced a significant decrease in the total costs of access for Cre-lox and Onco mice, while experiencing no shift in these costs for Knock-out and Spontaneous mice. These increases in openness provide the key source of variation we exploit in our empirical analysis. Three features of this increase in openness are particularly significant, and deserve elaboration. First, while the use of all genetically engineered mice was increasing over time, there is no evidence that the set of potential applications for Onco or Cre-lox mice was growing at a faster (or slower) rate than those for Knock-out or Spontaneous mice. All four categories of mice serve as broadly applicable inputs for follow-on research, with the most significant distinction stemming from the different patterns of access costs before and after the NIH agreements, as described above. Second, though the academic community lobbied continuously for increased openness regarding the Onco and Cre-lox mice, there is significant evidence that the precise timing and scope of the two NIH agreements were largely unanticipated.¹⁸ Given that academic lobbying efforts for easier access to these mice spanned nearly a decade, it is unlikely that researchers were simply shifting publication dates for already-performed research in anticipation of a comprehensive agreement eliminating reach-through rights. Instead, researchers deterred by the licensing restrictions imposed by DuPont undertook different research projects. The third important feature of the NIH agreements is their broad scope: they impacted more than 50 mice that had been developed and disclosed in the scientific literature using the Cre-lox technology, and more than 160 different Onco mice that were similarly disclosed. As we outline in detail below, the mice in both our treatment and control samples were developed and disclosed at different times during the pre-agreement period, and their use by follow-on researchers can be meaningfully captured by citations to the original mouse-articles.

¹⁸We discuss this point in more detail in Section 6.2 below.

1.4 Empirical strategy

1.4.1 Identification strategy

We examine the impact of a sudden reduction in access costs for genetically engineered mice (arising from the NIH agreements described above) on the level and composition of downstream research. Building on Furman and Stern (2008), our approach addresses a fundamental inference problem associated with traditional cross-sectional approaches to the evaluation of shifts in openness and related institutional arrangements: if more “open” inputs are used more extensively by downstream researchers, does this follow from the fact that they are open or from the fact that openness tends to be associated with higher-quality inputs and materials? Any effective estimation strategy must disentangle the selection effect (i.e., the correlation between openness and overall research impact) from the direct impact of openness.

Ideally, causal identification of the impact of openness would rely on a controlled experiment in which different knowledge inputs (such as particular research mice) are randomly allocated to distinct institutional environments with varying degrees of openness. A practical route capturing the essence of such an approach takes advantage of institutional variations that shift key research inputs towards higher (or lower) levels of access costs in a way that is exogenous both to their initial production and to their incorporation into downstream research lines.

We implement this idea by taking advantage of the institutional changes to openness negotiated by the NIH that affected some (but not all) research mice in our sample.¹⁹ As described in the previous section, new specialized research mice are disclosed through publication in scientific articles that describe their production and distinctive characteristics (we refer to these disclosures as mouse-articles). In constructing our sample, we identify mouse-articles for mice affected by the NIH agreements (i.e., Cre-lox and Onco mouse-articles) and for mice that were unaffected (i.e., Knock-Out and spontaneous mouse-

¹⁹Our approach builds on recent work applying a differences-in-differences econometric framework to analyze the institutional and microeconomic foundations of knowledge accumulation (Murray and Stern, 2007; Furman and Stern, 2008; Huang and Murray, 2008; Rysman and Simcoe, 2008).

articles).²⁰

In our analysis, we trace out the scientific impact of each mouse-article over time through the citations to that mouse-article by downstream research published in the scientific literature. While an imperfect and noisy indicator of overall impact, citations offer a systematic reflection of the process by which researchers acknowledge how their efforts at any given research stage build on the tools and knowledge developed by researchers in upstream stages. In the case of mouse-articles, our qualitative research suggests that citations to a given mouse-article involve the use of that article's specialized research mouse in a downstream experiment, and that downstream researchers almost always include a citation to the original mouse-article whenever its associated mouse is used in their project. Our analysis benefits from the fact that both Cre-lox and Onco NIH agreements occurred well after the initial development of these technologies; thus for each mouse-article in our sample, we are able to observe citations both before and after the NIH agreements. Finally, as noted above, the precise timing and scope of the openness shock were largely unanticipated. Specifically, the NIH agreement could have been reached, in principle, at any point in time from the early 1990s through the present. Moreover, our main control group – Knock-out mice – is likely to have been drawn from a population of similar scientific quality and importance, differing only insofar as the patent over Knock-out technology was unenforced by the University of Utah.²¹

In this empirical setting, we can estimate pre- and post-NIH agreement citation rates to the treated mouse-articles (those associated with Cre-lox and Onco mice). We also include untreated mouse-articles (Knock-out and Spontaneous mice) to estimate the counterfactual citation rate that would have occurred if the NIH agreement has not been signed. Overall, by measuring citations to Cre-lox and Onco mouse-articles before and after the sudden

²⁰While these different technologies differ in the precise details of the specialized genetic manipulation they allow, with the exception of Spontaneous mice, they are broadly similar with regard to the scope of application and relevance to human disease. Moreover, all three technologies were patented and could have been subject to strict enforcement. Spontaneous mice differ to the extent that they were not subject to patents.

²¹We find support for this view in our analysis of pre-NIH-agreement trends in Section 6.2 and Table 7. After controlling for mouse-article, age, and calendar-time fixed effects, we show that prior to their respective NIH agreement dates, the growth rates of citations to Cre-lox and Onco mice are statistically indistinguishable from those to Knock-out mice.

reduction in downstream access costs, and by measuring the citations to mouse-articles which experienced no shift in access costs, we can identify the causal impact of the increase in openness stemming from the Cre-lox and Onco NIH agreements.

1.4.2 Regression specifications

Our baseline regression takes the measure $Annual\ Citations_{jt}$ as its dependent variable, representing the number of citations to a given mouse-article j in calendar year t . On the RHS of the regression equation, we use $Post-NIH$ as our key treatment variable, equal to one for observation jt if the downstream research stemming from mouse-article j was impacted by an NIH agreement in year t . We use the variable $NIH-Window$ in the same manner to capture the period between the signing of the agreement and the point when the MoU would have a chance to impact publication behavior.²² Using a dataset of citations to mouse-articles impacted by the NIH agreement and mouse-articles that were unaffected, consider the following conditional fixed effects negative binomial estimator:

$$\begin{aligned} Annual\ Citations_{jt} = & f(\varepsilon_{jt}; \gamma_j + \beta_t + \delta_{t-PubYear} \\ & + \Psi_0 NIH-Window_{jt} + \Psi_1 Post-NIH_{jt}), \end{aligned} \quad (1.1)$$

where γ_j is a mouse-article fixed effect (conditioned out in estimation), β_t is a calendar-year fixed effect, and $\delta_{t-PubYear}$ is an age fixed effect calculated from the year in which mouse-article j was published. These controls account for the heterogeneity among the mouse-articles, the nonlinear evolution of citations over time elapsed from the initial publication of the mouse-article, and the potential for differences over time in citation rates. This specification also accounts for the incidental parameters problem (Hausman, Hall and Griliches, 1984), testing for the impact of the NIH agreements by estimating the *proportional change* in citation rate for mouse-articles in the treatment group in response to the NIH agreement, after accounting for the impact of our control variables, and relative to the untreated control groups.

²²Consistent with our description of life science research in Section 3, the window period for Cre-lox mice covers 1998 and 1999, and the window period for Onco mice covers 1999 and 2000.

We then turn to evaluating the impact of the increase in openness on the composition of downstream citations. Within each calendar year, for each mouse-article, we tabulate citations by key characteristics into two mutually exclusive types, and estimate the impact of the NIH agreement on each citation-year margin. For example, we predict that openness should increase the number of distinct downstream researchers utilizing a given research mouse. To test this hypothesis, we estimate the differential impact of a shift in openness on the number of authors publishing downstream research who have previously cited a particular mouse-article (*Old Authors_{jt}*) relative to the number of downstream researchers who have not previously cited that mouse-article (*New Authors_{jt}*).

Specifically, we jointly estimate the following two equations:

$$\begin{aligned} \text{New Authors}_{jt} = & f(\varepsilon_{jt}; \gamma_j + \alpha t + \beta_t + \delta_{t-PubYear}^{NEW} \\ & + \Psi_0^{NEW} NIH\text{-Window}_{jt} \\ & + \Psi_1^{NEW} Post\text{-}NIH_{jt}) \end{aligned} \quad (1.2)$$

and

$$\begin{aligned} \text{Old Authors}_{jt} = & f(\varepsilon_{jt}; \gamma_j + \beta_t + \delta_{t-PubYear}^{OLD} \\ & + \Psi_0^{OLD} NIH\text{-Window}_{jt} \\ & + \Psi_1^{OLD} Post\text{-}NIH_{jt}), \end{aligned} \quad (1.3)$$

where γ_j is a mouse-article fixed effect, α parametrizes a linear calendar-time-trend difference between the two types of citations, β_t is a calendar-year fixed effect, and $\delta_{t-PubYear}^{NEW}$ and $\delta_{t-PubYear}^{OLD}$ are mouse-article age fixed effects as in the previous specification. To evaluate whether the impact of the openness shock on downstream citations is concentrated in citations by authors who had not previously cited a particular mouse-article, we test whether $\Psi_1^{NEW} > \Psi_1^{OLD}$. This specification includes several parametric restrictions, including setting the mouse-article fixed effects γ_j and calendar-time fixed effects β_t to be equal across the two equations (1.2) and (1.3), and imposing a linear functional form (parametrized by α) on the difference in the effect of calendar time across the two equations. We do allow

for the mouse-article age fixed effects to vary freely across (1.2) and (1.3), as the evolution of citations in the time elapsed since publication may differ significantly for the two citation margins (in particular, most citations in the first few years after publication will be associated with *newauthors*).

We use a similar approach to evaluate whether a boost in citations is associated with (a) new versus old institutions, (b) new versus old key words, and (c) new versus old journals. Finally, we explore the downstream research response to the openness shocks by comparing citations to a given mouse-article in applied versus basic journals.

Our empirical framework also allows us to examine whether citations to mouse-articles in the treatment and control groups evolve in a similar way over time, except for shifts in the openness of the research environment. We can test this assumption directly by allowing for a calendar-time trend specific to the treatment group for each citation margin. Because different mouse-articles are published in different years we are able to disentangle the treatment effect of the NIH agreements from age- or calendar-time-based differences in the citation trends of articles in the treatment group. At the same time, we conjecture that the treatment effect should actually increase with the time elapsed from the openness shock, due to the higher likelihood of new downstream research lines being created from the original mouse-articles. We therefore include a specification which separately estimates the short-term and long-term impacts of the NIH agreements. Lastly, we can also test for an unanticipated increase in citations in the periods immediately preceding the NIH agreements, as a way to verify whether the timing of the openness shock was indeed unanticipated by downstream researchers. Specifically, we explore this possibility by testing for a pre-NIH treatment period in the years immediately prior to the signing of the NIH agreements.

1.5 Data

1.5.1 Data and sampling

The data for this study are drawn from the entire population of research mice catalogued by the Mouse Genome Informatics (MGI) database. MGI consists of over 13,000 unique

mice, each of which can be linked to a publication in the scientific literature describing its initial disclosure, thereby establishing a population of mouse-articles. Within this large population, we focus only on mouse-articles published between 1987 and 1998 (the date of the first NIH agreement). As outlined in Section 3, we sample all mouse-articles for the four major genetic engineering technologies defined by MGI: Cre-lox (28), Onco (102), Knock-out (1895), and Spontaneous (146). Our sample thus includes 2171 novel mice, each linked to a unique mouse-article.

We use PubMed and Thomson ISI Web of Science to collect detailed bibliometric information on all follow-on forward citations in academic journals through 2006. Each of these 432,083 citations includes information on last author, reprint author, institutional addresses, key words, and journal characteristics (including journal name, journal impact factor and a score for basicness). Citations are then aggregated into 22,265 citation-year observations by combining all the citations received by a given mouse-article in any particular year; this citation-year structure serves as the basis for our analysis.

To capture the *composition* of downstream research, we code citation characteristics into a set of mutually exclusive categorical variables. To illustrate the construction of these variables, take the case of new key words. For each downstream citation, ISI Web of Science provides a series of key words (referred to as Key Words Plus). We first take the list of all key words in the set of citations a particular mouse-article receives in a given year, and remove duplicates to obtain a list of unique key words associated with that citation-year observation. We then categorize a given key word to be new if it has never been used in citations to that particular mouse-article in any prior year, and code as old all key words that have appeared in citations in prior years. This construction allows us to capture changes in the research landscape over time. We generate four new/old categorical variables:

- i. New/Old Last Author: defined as new if the last author has never appeared as a last author before in a citation to the mouse-article in prior years; old otherwise.
- ii. New/Old Institution: defined as new if an address in the institution list has never appeared in an address list of citations to the mouse-article in prior years; old otherwise.

- iii. New/Old Key Words: defined as new if a key word has never before appeared in the key word list of citations to the mouse-article; old otherwise.
- iv. New/Old Journal: defined as new if the journal of the citation has never appeared before in the citations to the mouse-article; old otherwise.

We also categorize citations according to whether they are published in basic or applied journals.²³ This allows us to evaluate whether the two openness shifts in our study lead to downstream research focused primarily on applied experiments moving toward commercialization, or on basic experiments aimed at expanding the base of scientific knowledge.

The categorical measures described above reflect various ways in which openness can have an impact on downstream innovations. Using the two-equation framework described in Section 4, they allow us to test the hypothesis that lower downstream access costs lead to more diverse lines of research, pursued by a more diverse range of scientists. We also investigate whether openness is associated with more basic or applied downstream research.²⁴

1.5.2 Variables and summary statistics

Table 1 provides variable names and definitions and Table 2 reports summary statistics. The dependent variable in our initial set of regressions is *Annual Citations_{jt}* which measures the total number of citations received by mouse-article j in year t . The average of *Annual Citations_{jt}* is 19.41 (with a minimum of 0 and maximum of 336), highlighting the overall importance of mouse genetics research in this period. Because we observe citation-years from 1993 through 2006, the average *Citation Year_{jt}* is 2001. We also create an alternative dependent variable, *High Quality Citations_{jt}*, with mean equal to 4.3,

²³Our Basic/Applied Journal definition is based on work by Lim (2004), who created the measure by building on a classification scheme developed by CHI Research, Inc. According to Lim, “CHI awards each journal a score from zero to four. For the biomedical sciences, levels one through four correspond to clinical observation, clinical mix, clinical investigation and basic science.” It is worth noting that according to this measure, multidisciplinary journals are classified as “basic.”

²⁴It is worth noting that we do not examine the impact of openness on the academic/industry citation margin. The NIH agreements were directed specifically to public-sector researchers and 97.5% of all downstream citations have at least one of their authors affiliated with a public institution.

which captures downstream citations published in a “top 50” journal . We then construct a series of dependent variables based on the key categorical margins of interest:²⁵

- i. *New Last Authos_{jt}* and *Old Last Authors_{jt}*, with mean values equal to 11.7 and 3.9 respectively.
- ii. *New Institutions_{jt}* and *Old Institutions_{jt}*, with mean values equal to 17.5 and 10.2 respectively.
- iii. *New Keywords_{jt}* and *Old Keywords_{jt}*, with mean values equal to 74.9 and 55.4 respectively.
- iv. *New Journals_{jt}* and *Old Journals_{jt}*, with mean values equal to 7.9 and 6.2 respectively.
- v. *Basic Citations_{jt}* and *Applied Citations_{jt}*, with mean values equal to 9.2 and 7.4 respectively.

²⁵Note that the sum of the two means for a given annual citation margin need not add up to the mean annual citation count. First, due to data-matching issues we cannot always identify 100% of citations as belonging to one or the other margin; this leads to a sum lower than the mean annual citation count. Second, new/old margins focus on the count of unique instances of the characteristic in question; for example, if there are multiple citations from a particular journal to a mouse-article in a given year, we only count the first such citation. This also leads to a sum lower than the mean annual citation count. Finally, for the counts of institutions and key words, each citation contains multiple entries for these fields, leading to counts higher than the mean annual citation count. For example, in the case of key words, the sum of the margin means is just over 120, indicating that the average citation is associated with between 6 and 7 key words.

Table 1.1: Variables and Definitions

VARIABLE	DEFINITION	SOURCE
MOUSE-ARTICLE CHARACTERISTICS		
Publication Year	Year in which Mouse-Article j is published	PM
# Authors	Count of the number of authors of Mouse-Article j	PM
Total Citations	# of Citations to Mouse-Article j from its publication date through 2006	SCI
CITATION-YEAR CHARACTERISTICS		
Annual Citations	Count of all Citations to Mouse-Article j in Year t	SCI
High Quality Annual Citations	Count of Citations to Mouse-Article j in Year t where the journal of the citation is a top 50 journal based on impact factor rankings.	ISI
Citation Year	Year in which Citations are received	SCI
CITATION-YEAR MARGIN CHARACTERISTICS		
Basic Citations	Count of Citations to Mouse-Article j in Year t where the journal of the citation is a basic-research journal	CHIBasic
Applied Citations	Count of Citations to Mouse-Article j in Year t where the journal of the citation is an applied-research journal	CHIBasic
New X	Count of unique values of characteristic X of Citations to Mouse-Article j in Year t which are “new” and have not appeared in the Citations to Mouse-Article j in prior years.	
Old X	Count of unique values of characteristic X of Citations to Mouse-Article j in Year t which are NOT “new” and have appeared in the Citations to Mouse-Article j in prior years.	
X = Last Author	Last Author listed on the Citation	PM
X = Institution	Institutional Addresses listed on the Citation	PM
X = Key Word	Key Words listed on the Citation	PM
X = Journal	Journal listed on the Citation	PM
OPENNESS SHOCK CHARACTERISTICS		
Post-NIH	Dummy variable equal to 1 if Article j is associated with an openness agreement (Cre-Lox, Onco) which is in effect in year t .	MGI
NIH-Window	Dummy variable equal to 1 if Article j is associated with an openness agreement (Cre-Lox, Onco) which is in its initial period in year t .	MGI
MOUSE TECHNOLOGY CHARACTERISTICS		
Earliest Year	The publication year of the earliest mouse-article in the MGI database associated GM technology k .	MGI
Total Mice Created(1983 onward)	The total number of mice listed in the MGI database, with mouse-articles published from 1983 onward, associated with GM technology k .	MGI
MOUSE CREATION CHARACTERISTICS		
Annual Mouse Creation	The number of mouse-articles published in Year t which introduce mice created using GM technology k .	MGI
New Creation Journals	Count of unique Journals publishing mouse-articles of GM technology k in Year t , which are “new” and have not published mouse-articles for GM technology k in prior years.	PM
Old Creation Journals	Count of unique Journals publishing mouse-articles of GM technology k in Year t , which are NOT “new” and have published mouse-articles for GM technology k in prior years.	PM

Table 1.2: Summary Statistics

VARIABLE	N	MEAN	STD. DEV.	MIN	MAX
MOUSE-ARTICLE CHARACTERISTICS (N = 2,171 mouse-articles)					
Publication Year	2171	1995.58	2.34	1987	1998
# of Authors	2171	7.110	3.407	1	34
Total Forward Citations	2171	210.8	230.4	1	2543
CITATION-YEAR CHARACTERISTICS (N = 22,265 citation-year observations)					
Citation Year	22265	2001.19	3.26	1993	2006
Annual Citations	22265	19.41	21.67	0	336
High Quality Annual Citations	22265	4.313	5.868	0	71
CITATION-YEAR MARGIN CHARACTERISTICS (N = 22,265 citation-year observations)					
Basic Citations	22265	9.176	11.239	0	151
Applied Citations	22265	7.446	10.778	0	157
New Last-Authors	22265	11.708	13.479	0	243
Old Last-Authors	22265	3.941	5.349	0	58
New Institutions	22265	17.541	17.829	0	287
Old Institutions	22265	10.212	13.657	0	135
New Key Words	22265	74.878	67.277	0	794
Old Key Words	22265	55.371	61.115	0	620
New Journals	22265	7.911	7.842	0	94
Old Journals	22265	6.168	7.608	0	81
OPENNESS AGREEMENT CHARACTERISTICS (N = 22,265 citation-year observations)					
Post-NIH	22265	0.036	0.187	0	1
NIH-Window	22265	0.011	0.105	0	1
Post-Cre-lox	22265	0.009	0.093	0	1
Cre-lox-Window	22265	0.002	0.045	0	1
Post-Onco	22265	0.027	0.164	0	1
Onco-Window	22265	0.009	0.095	0	1
MOUSE CREATION CHARACTERISTICS (N = 78 technology-year observations)					
Annual Mouse Creation	78	108.192	171.586	0	875
New Creation Journals	78	6.987	5.805	0	24
Old Creation Journals	78	16.128	18.129	0	75

Next, we define two measures that will be used to estimate the impact of the NIH agreements. We divide the period after the NIH agreement signing into two subperiods because

the sudden increase in openness would likely take time to influence downstream research. Specifically, we define a window period and a treatment period to allow a reasonable lag (2 years) for the NIH agreement to impact observed citation patterns. *NIH-Window* (mean equal to 0.011) is a dummy variable equal to one for articles impacted by an NIH agreement during the year when that agreement was signed and during the following year (1998/1999 for Cre-lox mouse-articles, 1999/2000 for Onco mouse-articles). Our key treatment variable, *Post-NIH_{jt}* (mean equal to 0.036), is a dummy variable equal to one for all articles impacted by the NIH agreements in years after the window period ended. Using the same approach, we also define separate treatment variables for the two NIH agreements: *Cre-lox-Window_{jt}*, *Post-Cre-lox_{jt}*, *Onco-Window_{jt}* and *Post-Onco_{jt}*. Finally, to examine the short-term versus long-term impact of the NIH agreements, we also define a treatment variable, *Post-NIH, Short-Term* equal to one for the first three years after the window period for affected mouse-articles, and a separate measure, *Post-NIH, Long-Term*, equal to one for the four years or more after the end of the window period.

We highlight our disaggregated summary statistics by the type of mouse technology in Table 3. The most salient point to note is that compared to the overall sample mean of 18, the *Annual Citations_{jt}* for Cre-lox and Onco mice are 15 and 12 respectively. However, the Spontaneous mice in our control group have a lower mean of 4, the Knock-out mice have mean *Annual Citations_{jt}* of over 21. This is consistent with the assumption of comparability between the treatment and control groups. Moreover, the mean publication year and mean number of authors across the four mouse technologies are similar.

Table 1.3: Summary Statistics by Mouse Technology

	MOUSE TECHNOLOGY			
VARIABLE	CRE-LOX	ONCO	KNOCK-OUT	SPONTANEOUS
MOUSE-ARTICLE CHARACTERISTICS (N = 2,171 mouse-articles)				
# of Mouse Articles	28	102	1895	146
Publication Year	1996.7	1993.4	1995.8	1993.5
# Authors	5.250	6.020	7.353	5.075
Total Citations	144.43	167.78	226.25	52.73
CITATION-YEAR CHARACTERISTICS (N = 22,265 citation-year observations)				
Annual Citations	14.719	12.297	21.285	3.955
MOUSE TECHNOLOGY CHARACTERISTICS (N = 4 technologies)				
Earliest Year	1992	1983	1989	1915
Total Mice Created(1983 onward)	1159	401	5980	911
MOUSE CREATION CHARACTERISTICS (N = 78 technology-year observations)				
Annual Mouse Creation	28.429	50.217	351.471	37.958
New Creation Journals	5.000	5.609	13.412	4.917
Old Creation Journals	7.286	14.783	37.765	7.250

1.6 Results

Now we proceed to estimate the causal impact of the NIH agreement openness shocks on the overall flow of citations (Table 4), and then turn to the core of our analysis which examines the impact of the NIH agreements on the composition of citations. Specifically, we examine the impact of openness on the type -new versus old- of researchers (Table 5) and the nature -new versus old- of research directions (Table 6). We then undertake robustness checks to allow for differences in the time trends of citations between our treatment and control groups (Table 7). Lastly, we analyze the impact of openness on the vertical exploitation of particular research lines by examining the composition of follow-on research in basic versus applied journals (Table 8). By adopting a differences-in-differences approach throughout our analysis to evaluate the impact of openness on different citation

margins, we are able to infer the relationship between openness and academic freedom.

In all our Tables we report coefficients estimates and their corresponding incidence-rate ratios (IRRs). We discuss our results in terms of IRRs because they are easily interpreted as percentage changes relative to a baseline (i.e. the null hypothesis of no effect yields a coefficient of 1.0). Also, all of our specifications report block bootstrapped standard errors clustered by mouse-article (MacKinnon, 2002).

1.6.1 Impact of openness on the level of follow-on research

Our regression results begin in Table 4 with a set of conditional fixed effect specifications focusing on the impact of our openness shocks on the quantity of downstream citations to the mouse-articles in our sample. The first column, (4-1), reports a conditional fixed effect OLS specification using the natural log of $Annual\ Citations_{jt}$ as the dependent variable. The remaining columns, (4-2) to (4-5), report results from conditional fixed effect negative binomial specifications, using the raw count of $Annual\ Citations_{jt}$ as the dependent variable. We move to the negative binomial specification for two reasons: first, because our dependent variable is based on discrete counts rather than continuous outcomes, and second, because a negative binomial specification allows for unbiased estimates of standard errors when there is unobserved variation in the underlying rates of the measured outcome. All specifications also include the full set of article, age and calendar-year fixed effects. In (4-1) and (4-2), we include both the *NIH-Window* and the *Post-NIH* regressors. The OLS specification in the first column serves as a point of comparison, and shows that the OLS and negative binomial results are similar in terms of both economic and statistical significance²⁶. Focusing on regression (4-2), the results are striking: after accounting for the window period, mouse articles impacted by an NIH agreement experience a 30% increase in their annual citation rate. As illustrated in (4-3), the impact of the NIH agreements is increasing over time: While the increase in citations in the three years after the window period is equal to 22%, the coefficient on *Post-NIH, Long-Term* suggests

²⁶As predicted by econometric theory, the OLS specification under-estimates the standard errors in our empirical setting. This effect is only somewhat mitigated when using bootstrapped standard errors. Throughout our analysis, we therefore use the larger standard errors calculated using the negative binomial specification.

that the permanent effect is nearly doubled (at 43%). Not simply a reflection of publication lags, the results in (4-3) suggest the presence of a positive and permanent increase in the use of genetically engineered mice which have been shifted to a higher level of openness. In (4-4) we estimate separate coefficients for the Cre-lox and Onco NIH agreements: Both are statistically significant although the magnitude of the boost to citations associated with the Cre-lox agreement is larger (47% for citations to Cre-lox mouse-articles compared to 27% for citations to Onco mouse-articles). Finally, in (4-5), we undertake a robustness check by focusing on citations in “high impact” journals. We find a 41% boost in high quality citations suggesting that the impact of the shift in openness is concentrated in research appearing in the most prestigious journals.

The results in Table 4 provide strong support for the hypothesis that positive shocks to openness foster follow-on research. These findings reinforce previous studies of the impact of openness and accessibility such as Furman and Stern (2009) and Murray and Stern (2007). Furthermore, our results are consistent with a multi-staged view of innovation whereby an increase in openness does not simply lead to a temporary increase in follow-on research but also has an increasing impact over time. Finally, though we hold off on this discussion until Table 7, we can show that the estimated impact of the NIH agreements is not simply due to a different time trend for the treatment and control groups. Taken together, these results highlight the sensitivity of follow-on researchers to the degree of openness of critical research inputs.

Table 1.4: Impact of Openness on Follow-On Research Flows

	Dep Var = ANNUAL CITATIONS [Incident rate ratios reported in square brackets] Estimated coefficients in 2nd line. (Block bootstrapped SEs reported in parentheses)				
	OLS	NEGATIVE BINOMIAL			
	(4-1) Baseline Model, DV = Log Annual Citations	(4-2) Baseline Model	(4-3) Baseline Model with Treatment Effect Dynamics	(4-4) Treatment Effects by Cre-lox &Onco	(4-5) Baseline Model, Citations from High Quality Journals only [#]
Post-NIH	[1.229]*** 0.206 (0.052)	[1.302]*** 0.264 (0.062)			[1.409]*** 0.343 (0.080)
Post-NIH, Short-term ⁺⁺			[1.220]*** 0.199 (0.064)		
Post-NIH, Long-term ⁺⁺⁺			[1.429]*** 0.357 (0.074)		
Post-Cre-lox				[1.467]*** 0.383 (0.115)	
Post-Onco				[1.267]*** 0.236 (0.060)	
CONTROL VARIABLES					
- NIH-Window+	[1.132]** 0.124 (0.049)	[1.146]** 0.136 (0.065)	[1.149]** 0.139 (0.058)	-	[0.954] -0.047 (0.092)
- Cre-lox-Window	-	-	-	[1.069] 0.067 (0.089)	-
- Onco-Window	-	-	-	[1.188]*** 0.172 (0.043)	-
Age FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Article FEs	Yes	Yes	Yes	Yes	Yes
Log-likelihood	-	-55919.8	-55906.1	-55912.4	-34112.8
# of Observations	22265	22265	22265	22265	21574

Significance levels: * 10% ** 5% *** 1%

+ Window is defined as the year of the NIH agreement and the following year (Cre-lox: 1998/1999; Onco: 1999/2000)

++ Short-term is defined as the three years following the window after the NIH agreement (Cre-lox: 2000-2002; Onco: 2001-2003).

+++ Long-term is defined as the years following the window and the short-term period after the NIH agreement (Cre-lox: 2003 onward; Onco: 2004 onward).

[#] For this regression we use a modified dependent variable that captures only those annual citations that appear in a subset of high quality journals, as ranked by ISI impact factor.

Tests of Differences Between Coefficients:

(4-2): $\beta(\text{Post-NIH}) - \beta(\text{NIH-Window})$:Estimate = **0.129**; SE = **0.033**; Prob>|z|<**0.001**(4-3): $\beta(\text{Post-NIH, Long-term}) - \beta(\text{Post-NIH, Short-term})$:Estimate = **0.158**; SE = **0.040**; Prob>|z|<**0.001**

1.6.2 Impact of openness on the level of follow-on exploration

Tables 5 and 6 present our main evidence to the effect that greater openness results in greater horizontal experimentation, spawning a more diverse array of research lines and encouraging the participation of new researchers. In Table 5, our key comparison is between researchers listed as the last author (senior scientist) who have (or have not) been previously listed on a citation to the mouse-article of interest, as captured in our measures *New Authors* and *Old Authors*. In (5-1a) and (5-1b) we estimate whether the marginal impact of *Post-NIH* is different for new versus old last-authors. While there is only an insignificant 13% increase in citations by old authors, the increase by new authors is estimated to be significant and more than 38%. Moreover, these two coefficients are significantly different from each other. We then estimate a separate coefficient for the short-term versus long-term impact of the NIH agreements on new versus old authors (5-2a and 5-2b). The increase in citations by new authors is greater than the increase in citations for old authors in both the short- and long-term (with the difference between the two coefficients being significant at the 1% level). Strikingly, the estimate of the long-term increase in new author citations is above 50%. Next, when we separately estimate the impact of the Cre-lox and Onco agreements on new versus old authors, (5-3a and 5-3b) we find that the estimated boost for new authors is statistically significant for each agreement compared to a much smaller and statistically insignificant increase in citations by old authors. Moreover, we find that the difference between the new versus old coefficients is significant for each agreement at the 5% level.

Figure 1.1 presents these effects graphically. Specifically, we plot the evolution of citations by new and old “last authors” for Cre-lox and Onco mouse-articles, relative to their pre-shock baselines. As in the regressions, we find that citations by new authors increase much more strongly than those by old authors, and that this gap is greater in the long-run than in the short-run. The result holds for both openness shocks, though the impact for Cre-lox mouse-articles is stronger because they were more difficult to obtain prior to their respective NIH agreement.

Finally, in (5-4a) and (5-4b) we turn to an alternative measure of the diversity of follow-on researchers as captured by their institutional affiliation. Similar to the results for new

versus old authors, the boost in citations associated with the NIH agreements is concentrated in citations from institutions that had not previously cited that mouse article (27% vs. 13% boost).

Overall, the results in Figure 1.1 and Table 5 provide direct evidence that the shift in openness associated with the NIH agreements expanded the diversity of researchers drawing on a particular line of research.

Figure 1.1: Impact of Openness on Citations by New vs. Old Last Authors

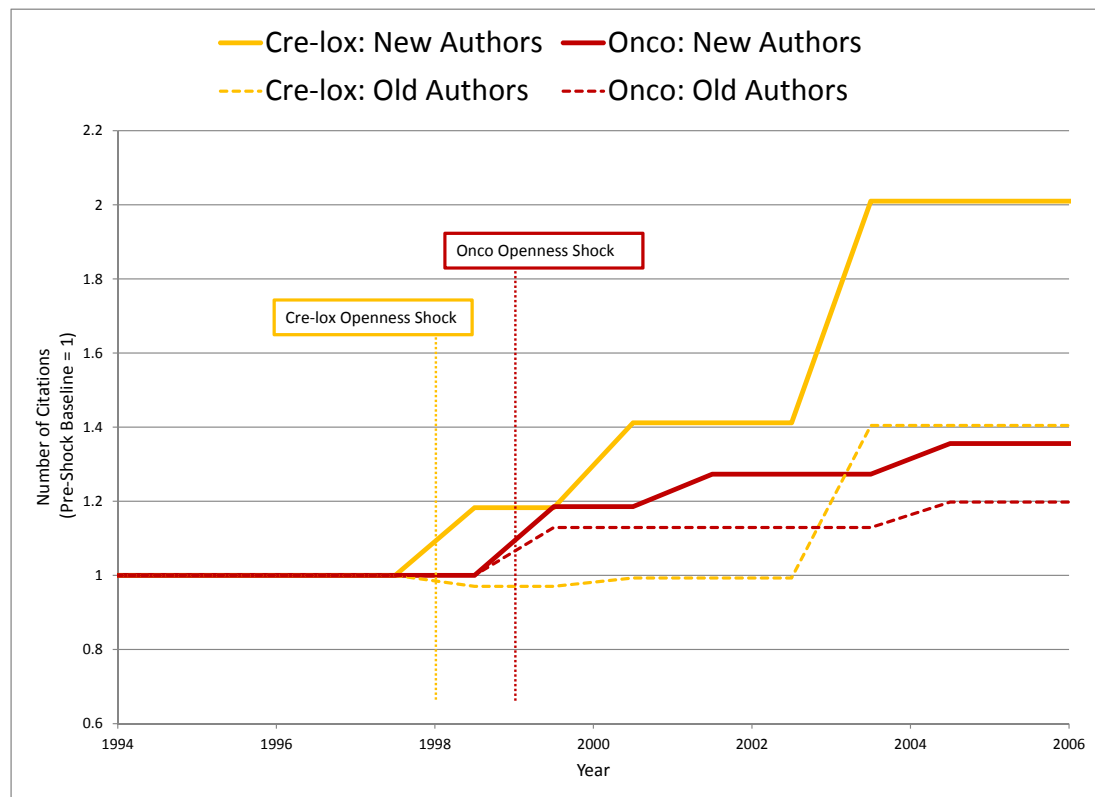


Table 1.5: Impact of Openness on Citations by New vs. Old Last Authors and New vs. Old Institutions

	STACKED NEGATIVE BINOMIAL [Incident rate ratios reported in square brackets] Estimated coefficients in 2nd line. (Block bootstrapped SEs reported in parentheses)							
	(5-1a) DV= New Authors	(5-1b) DV= Old Authors	(5-2a) DV= New Authors	(5-2b) DV= Old Authors	(5-3a) DV= New Authors	(5-3b) DV= Old Authors	(5-4a) DV= New Institutions	(5-4b) DV= Old Institutions
Post-NIH	[1.379]*** 0.321 (0.065)	[1.135] 0.127 (0.088)					[1.269]*** 0.238 (0.052)	[1.127]* 0.120 (0.066)
Post-NIH, Short-term			[1.276]*** 0.244 (0.062)	[1.064] 0.062 (0.078)				
Post-NIH, Long-term			[1.537]*** 0.430 (0.071)	[1.224]*** 0.202 (0.073)				
Post-Cre-lox					[1.649]** 0.500 (0.203)	[1.189] 0.173 (0.211)		
Post-Onco					[1.305]*** 0.266 (0.076)	[1.160] 0.148 (0.108)		
CONTROL VARIABLES								
Window FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes ⁺		Yes ⁺		Yes ⁺		Yes ⁺	
Article FEs	Yes		Yes		Yes		Yes	
Log-likelihood	-86889.3		-86874.1		-86877.2		-114094.0	
# of Observations	42802		42802		42802		42830	

Significance levels: * 10% ** 5% *** 1%

+ Calendar year fixed effects include a set of indicator variables common to both margins in a given regression, and a linear difference variable which allows for a constant difference in growth rates between the two margins.

Tests of Differences Between Coefficients:

(5-1): $\beta(\text{Post-NIH effect on New Authors}) - \beta(\text{Post-NIH effect on Old Authors}):$

Estimate = **0.194**; *SE* = **0.042**; *Prob>|z|* < **0.001**

(5-2): $\beta(\text{Post-NIH, Short-term effect on New Authors}) - \beta(\text{Post-NIH, Short-term effect on Old Authors}):$

Estimate = **0.181**; *SE* = **0.047**; *Prob>|z|* < **0.001**

$\beta(\text{Post-NIH, Long-term effect on New Authors}) - \beta(\text{Post-NIH, Long-term effect on Old Authors}):$

Estimate = **0.227**; *SE* = **0.042**; *Prob>|z|* < **0.001**

(5-3): $\beta(\text{Post-Cre-lox effect on New Authors}) - \beta(\text{Post-Cre-lox effect on Old Authors}):$

Estimate = **0.327**; *SE* = **0.064**; *Prob>|z|* < **0.001**

$\beta(\text{Post-Onco effect on New Authors}) - \beta(\text{Post-Onco effect on Old Authors}):$

Estimate = **0.118**; *SE* = **0.054**; *Prob>|z|* = **0.029**

(5-4): $\beta(\text{Post-NIH effect on New Institutions}) - \beta(\text{Post-NIH effect on Old Institutions}):$

Estimate = **0.118**; *SE* = **0.035**; *Prob>|z|* < **0.001**

In Table 6 we turn to the related prediction that openness enhances the diversity of research lines (particularly in an academic research environment where scientists are free to choose their own research direction). We capture the degree of horizontal diversity by

using the key words that categorize each citation (recall that key words are chosen by the archiving service rather than the researchers). In (6-1a) and (6-1b) we compare the impact of the NIH agreements on *New Key Words* and *Old Key Words* respectively. While there is a small and statistically insignificant decline in the number of old key words there is a significant 26% increase in the number of citations with new key words. Moreover, these coefficients are statistically significantly different from each other. This is not just a short-term effect: the analysis of time dynamics in (6-2a) and (6-2b) indicates that there is an even larger 41% increase in the number of new key works in the long-term, relative to an insignificant decrease to old key words in the long-term; moreover, the difference between this 41% increase and the 18% increase to new key words in the short-term is statistically significant at the 1% level. When we decompose the openness changes into the Cre-lox and Onco agreements (see 6-3a and 6-3b), we continue to find a quantitatively and statistically significant difference between the new and old key words coefficients. Both the Cre-lox and Onco agreements are associated with a significant boost in new key words (40% and 21% respectively) and a small and insignificant decline in old key words.

In Figure 1.2, we present the key word results in a graphical format. As in Figure 1.1, we plot citations containing new and old key words for both Cre-lox and Onco mice, relative to their pre-shock baselines. For Cre-lox mice, we see a small short-term drop in all key words, followed by a return to pre-shock levels for old key words and a strong increase in new key words. For Onco mice, we also see a drop in old key words followed by a slow return to pre-shock levels, while new key words experience an immediate and permanent increase. For both technologies, there is a strong shift away from old and toward new key words following the openness shocks.

Finally, as in our analysis of the diversity of citing researchers, we use an alternative measure to test the robustness of our findings on research diversity. In (6-4a) and (6-4b) we compare the citation margins between *New Journals* and *Old Journals*, where a “new” journal is one which has never before published an article citing the original mouse-paper article in question. We find that being in the *Post-NIH* period leads to a 38% increase (significant at the 1% level) in citations from new journals, and only a 20% increase in citations from old journals (significant at the 5% level).

Figure 1.2: Impact of Openness on Citations with New vs. Old Key Words

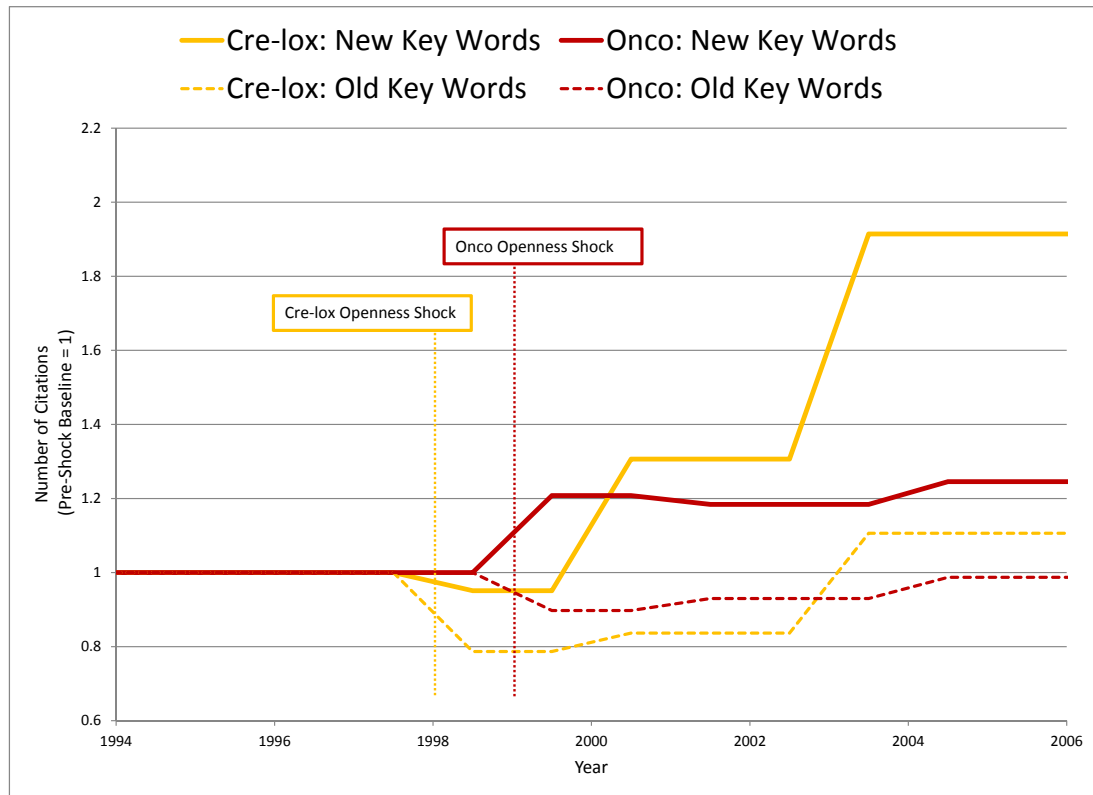


Table 1.6: Impact of Openness on Citations with New vs. Old Key Words

	STACKED NEGATIVE BINOMIAL [Incident rate ratios reported in square brackets] Estimated coefficients in 2nd line. (Block bootstrapped SEs reported in parentheses)							
	(6-1a) DV=New Key Words	(6-1b) DV=Old Key Words	(6-2a) DV= New Key Words	(6-2b) DV= Old Key Words	(6-3a) DV=New Key Words	(6-3b) DV=Old Key Words	(6-4a) DV=New Journals	(6-4b) DV=Old Journals
Post-NIH	[1.260]*** 0.231 (0.070)	[0.925] -0.078 (0.075)					[1.381]*** 0.323 (0.076)	[1.201]** 0.183 (0.084)
Post-NIH, Short-term			[1.178]*** 0.164 (0.061)	[0.882]* -0.126 (0.066)				
Post-NIH, Long-term			[1.405]*** 0.340 (0.070)	[0.989] -0.011 (0.071)				
Post-Cre-lox					[1.399]* 0.336 (0.202)	[0.879] -0.129 (0.194)		
Post-Onco					[1.208]*** 0.189 (0.062)	[0.955] -0.046 (0.076)		
CONTROL VARIABLES								
Window FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes ⁺		Yes ⁺		Yes ⁺		Yes ⁺	
Article FEs	Yes		Yes		Yes		Yes	
Log-likelihood	-179179.1		-179162.5		-179146.0		-88007.3	
# of Observations	44488		44488		44488		42830	

Significance levels: * 10% ** 5% *** 1%

+ Calendar year fixed effects include a set of indicator variables common to both margins in a given regression, and a linear difference variable which allows for a constant difference in growth rates between the two margins.

Tests of Differences Between Coefficients:

(6-1): $\beta(\text{Post-NIH effect on New Key Words}) - \beta(\text{Post-NIH effect on Old Key Words})$:
Estimate = **0.310**; *SE* = **0.038**; *Prob>|z|* < **0.001**

(6-2): $\beta(\text{Post-NIH, Short-term effect on New Key Words}) - \beta(\text{Post-NIH, Short-term effect on Old Key Words})$:
Estimate = **0.290**; *SE* = **0.038**; *Prob>|z|* < **0.001**
 $\beta(\text{Post-NIH, Long-term effect on New Key Words}) - \beta(\text{Post-NIH, Long-term effect on Old Key Words})$:
Estimate = **0.351**; *SE* = **0.035**; *Prob>|z|* < **0.001**

(6-3): $\beta(\text{Post-Cre-lox effect on New Key Words}) - \beta(\text{Post-Cre-lox effect on Old Key Words})$:
Estimate = **0.466**; *SE* = **0.059**; *Prob>|z|* < **0.001**
 $\beta(\text{Post-Onco effect on New Key Words}) - \beta(\text{Post-Onco effect on Old Key Words})$:
Estimate = **0.235**; *SE* = **0.039**; *Prob>|z|* < **0.001**

(6-4): $\beta(\text{Post-NIH effect on New Journals}) - \beta(\text{Post-NIH effect on Old Journals})$:
Estimate = **0.140**; *SE* = **0.043**; *Prob>|z|* = **0.001**

In our analysis so far we have assumed that the citation-age profile is similar for the treatment and control groups. In Table 7 we re-estimate each of the key equations for overall citations, new versus old authors, and new versus old key words, allowing for a time trend specific to the treatment group for each citation margin. Since the treatment

effect itself is predicted to increase in the time elapsed since the agreement we separately allow for a post-NIH-agreement trend. The results reinforce our overall findings. First and most importantly, across all of the specifications there is no statistically significant or quantitatively important trend specific to the treatment articles. Second, the estimated coefficients for the impacts of the NIH agreements remain significant, although smaller than in the previous tables, presumably because they now capture the impact of the openness shock only for the first year after the window period. More importantly, there is a significant impact of the treatment over time for overall citations, new authors and new key words. While there is also an increase over time for old authors and old key words, the coefficient is smaller and noisier.

We have also experimented extensively with specifications that estimate coefficients on a year-by-year basis relative to the time of the NIH agreements, in order to test for the presence of a pre-shock trend in either of the treatment groups (relative to controls) and to examine the evolution of each citation margin after the shock. While the pre-deposit trend is not statistically significant for any of the citation margins we consider, it is also true that these year-by-year coefficients are imprecisely estimated, in part because of the relatively small number of annual citation-year observations in the treatment groups. In other words, there is no evidence of a significant increase in citations prior to the NIH agreements that might raise concerns about the endogeneity of the timing of the agreement.

Table 1.7: Robustness Tests for a Pre-Shock Treatment Trend

	[Incident rate ratios reported in square brackets] Estimated coefficients in 2 nd line. (Block bootstrapped SEs reported in parentheses)				
	NEGATIVE BINOMIAL	STACKED NEGATIVE BINOMIAL			
	(7-1) DV= Annual Citations With Treatment Trends	(7-2a) DV= New Authors With Treatment Trends	(7-2b) DV= Old Authors With Treatment Trends	(7-3a) DV= New Keywords With Treatment Trends	(7-3b) DV= Old Keywords With Treatment Trends
Post-NIH	[1.145]* 0.135 (0.078)	[1.117] 0.111 (0.091)	[1.034] 0.033 (0.078)	[1.127] 0.120 (0.102)	[0.984] -0.016 (0.118)
Treatment Group Age Trend per Year	[1.003] 0.003 (0.015)	[1.014] 0.014 (0.018)	[1.000] -0.000 (0.020)	[1.001] 0.001 (0.025)	[0.997] -0.003 (0.029)
Post-NIH Change in Trend per Year	[1.050]*** 0.049 (0.017)	[1.052]** 0.051 (0.025)	[1.046]* 0.045 (0.025)	[1.053]** 0.052 (0.025)	[1.045] 0.044 (0.028)
CONTROL VARIABLES					
NIH-Window	[1.114]** 0.108 (0.047)	[1.079] 0.076 (0.062)	[1.091] 0.087 (0.071)	[1.081] 0.078 (0.068)	[0.860]** -0.151 (0.063)
Age FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes ⁺		Yes ⁺	
Article FEs	Yes	Yes		Yes	
Log-likelihood	-55899.5		-86859.4		-179152.1
# of Observations	22265		42802		44488

Significance levels: * 10% ** 5% *** 1%

+ Calendar year fixed effects include a set of indicator variables common to both margins in a given regression, and a linear difference variable which allows for a constant difference in growth rates between the two margins.

Tests of Differences Between Coefficients:

(7-2): $\beta(\text{Post-NIH effect on New Authors}) - \beta(\text{Post-NIH effect on Old Authors})$:
Estimate = 0.078; SE = 0.077; Prob>|z|=0.312

(7-3): $\beta(\text{Post-NIH effect on New Key Words}) - \beta(\text{Post-NIH effect on Old Key Words})$:
Estimate = 0.277; SE = 0.056; Prob>|z|<0.001

1.6.3 Impact of openness on basic and applied follow-on research

In Table 8 we turn to the effects of openness shocks on the distribution of research along the development spectrum, ranging from early-stage basic science to later-stage applications of the preceding innovations. We do so by examining the marginal impact of the openness shocks on the production of research in basic versus applied research journals. In (8-1a) and (8-1b), we find that the *Basic Citations* dependent variable increases by 26% during the post-NIH-agreement period; at the same time, *Applied Citations* experience

a 30% increase during that period (both significant at the 1% level). This suggests that the overall impact of the NIH agreements involves both basic and applied citations. We then disentangle the separate impacts of the Cre-lox and Onco agreements. Recall that in the pre-openness period, not only were there stringent reach-through rights associated with Cre-lox mice, but also very limited access to the mice due to ex ante enforcement by DuPont. In contrast, Onco mice were made available in the pre-openness period through the Jackson Labs. As a result, relative to the Cre-lox MoU the Onco MoU has had a bigger relative impact in terms of reducing reach-through rights rather than on access costs. In (8-2a) and (8-2b) we evaluate the differential impact of these two NIH agreements on basic versus applied citations. We find that the impact of the Cre-lox agreement is concentrated in basic citations, whereas the Onco shock has a significant effect only on applied citations. Specifically, the Cre-lox agreement leads to a 120% increase in basic citations (significant at the 1% level) but no change in applied citations, whereas the Onco agreement leads to a 57% increase in applied citations but has no significant impact on basic citations. This is consistent with the view that when direct access to upstream inputs is already secured (as was the case for Onco mice), then an agreement that shifts the balance of IP rights toward downstream innovators induces more applied research.

Table 1.8: Impact of Openness on Citations in Basic vs. Applied Journals

	STACKED NEGATIVE BINOMIAL [Incident rate ratios reported in square brackets] Estimated coefficients in 2nd line. (Block bootstrapped SEs reported in parentheses)			
	(8-1a) DV= Basic Journal Citations	(8-1b) DV= Applied Journal Citations	(8-2a) DV= Basic Journal Citations	(8-2b) DV= Applied Journal Citations
Post-NIH	[1.262]*** 0.233 (0.066)	[1.301]*** 0.263 (0.061)		
Post-Cre-lox			[2.212]*** 0.794 (0.126)	[1.073] 0.070 (0.105)
Post-Onco			[1.076] 0.073 (0.062)	[1.565]*** 0.448 (0.075)
CONTROL VARIABLES				
Window FEs	Yes	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes	Yes
Year FEs	Yes ⁺		Yes ⁺	
Article FEs	Yes		Yes	
Log-likelihood	-105989.0		-105894.7	
# of Observations	44530		44530	

Significance levels: * 10% ** 5% *** 1%

+ Calendar year fixed effects include a set of indicator variables common to both margins in a given regression, and a linear difference variable which allows for a constant difference in growth rates between the two margins.

Tests of Differences Between Coefficients:

(8-1): $\beta(\text{Post-NIH effect on Basic Journal Citations}) - \beta(\text{Post-NIH effect on Applied Journal Citations})$:
Estimate = -0.030; *SE* = 0.072; *Prob>|z|* = 0.676

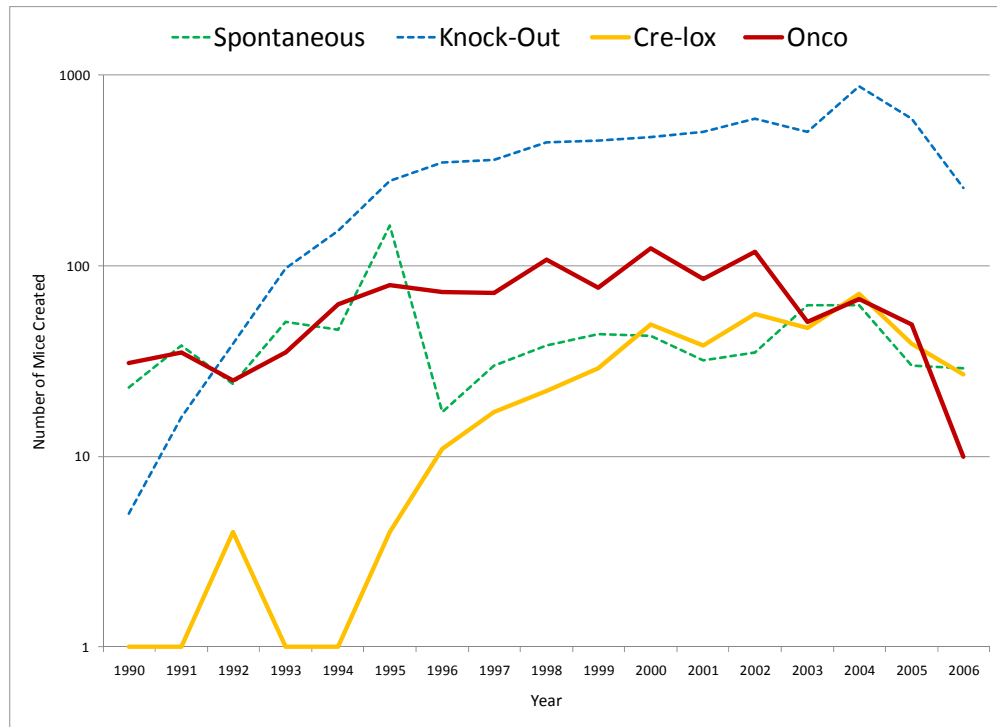
(8-2): $\beta(\text{Post-Cre-lox effect on Basic Journal Citations}) - \beta(\text{Post-Cre-lox effect on Applied Journal Citations})$:
Estimate = 0.724; *SE* = 0.122; *Prob>|z|* < 0.001
 $\beta(\text{Post-Onco effect on Basic Journal Citations}) - \beta(\text{Post-Onco effect on Applied Journal Citations})$:
Estimate = -0.375; *SE* = 0.086; *Prob>|z|* < 0.001

1.6.4 Impact of openness on the creation of new genetically modified mice

Finally, we analyze the impact of the Cre-lox and Onco openness shocks on the rate of creation of genetically modified mice. Our empirical approach begins with the full set of mice in the MGI database (approximately 13,000). For each mouse and its associated mouse-article, we obtain information regarding the technology used in its creation, and the journal in which the article was published. We focus on mice created since 1983, using one

of the four technologies present in our citation analysis. Figure 1.3 describes the creation of mice by technology category, from 1990 through 2006, in order to provide a visual representation of any potential impact on mouse creation of the NIH agreements for Cre-lox and Onco mice. A standard incentive story might predict a reduction in the creation of Cre-lox and Onco mice, relative to the control technologies, following the corresponding NIH agreements. However, the graph does not exhibit any marked change in creation rates around the time of the two agreements (1998 and 1999, respectively).

Figure 1.3: Mouse Creation by Technology



Next, we perform a differences-in-differences regression similar to those in our citation analysis. Specifically, our baseline regression takes the measure $Annual\ Mouse\ Creation_{kt}$ as its dependent variable, representing the number of mouse-articles published to a given technology k in a given calendar year t . Our key regressors are interaction effects between each of the NIH agreements and the Annual Mouse Creation years impacted by those agreements: $Cre-lox-Window_{kt}$, $Post-Cre-lox_{kt}$, $Onco-Window_{kt}$ and

Post-Onco_{kt}. We also include controls for calendar-year, age, and technology-specific effects. Similar to our earlier specifications, we include a technology fixed effect (conditioned out in the context of our conditional fixed effect negative binomial estimator), calendar year fixed effects, and a cubic polynomial in the age of the technology. Finally, because of the much older age of the Spontaneous mice, we include a separate linear time trend for the Spontaneous group. This results in the following specification:

$$\begin{aligned}
 \text{Annual Mouse Creation}_{kt} = & f(\varepsilon_{kt}; \gamma_k + \beta_t + g(t - \text{EarliestYear}_k) \\
 & + \sigma I(k = \text{"Spontaneous"}) * (t - \text{EarliestYear}_k) \\
 & + \Psi_{0,1} \text{Cre-lox-Window}_{kt} \\
 & + \Psi_{1,1} \text{Post-Cre-lox}_{kt} \\
 & + \Psi_{0,2} \text{Onco-Window}_{kt} \\
 & + \Psi_{1,2} \text{Post-Onco}_{kt}),
 \end{aligned} \tag{1.4}$$

Overall, this specification tests for the impact of changes in openness on mice creation by estimating how the mice-creation rate for a technology *changes* in response to corresponding the NIH agreements, accounting for fixed differences in creation rates across technologies and relative to the trend in creation rates for the non-treated control groups.

Our results of this differences-in-differences specification are shown in the first two columns of Table 9. In (9-1), we use the four technologies we studied in our citation analysis; the estimates suggest that, rather than a decrease, there was a 50% increase in mouse creation after the NIH agreements for both Cre-lox and Onco mice. However, this estimated increase is a fragile result. In (9-2), we expand the control group to include all major genetic modification technologies in the MGI database, adding the following four technologies: Targeted(reporter), Targeted(knock-in), Targeted(Floxed/Frt), and Genetrapped mice. The strong increase in mouse creation reported in (9-1) largely disappears when we include a wide set of control groups. In particular, for the Onco technology, we see virtually no change in mouse creation; for Cre-lox, we find an increase of over 20% in mouse creation, but this result is no longer statistically significant. However, both (9-1) and (9-2) offer an important take-away: *increased openness does not lead to a reduction in the overall cre-*

ation of new research mice. This contradicts the view that increased openness on upstream research should discourage the development of new mice. Instead, consistent with our emphasis on exploration in the process of innovation, a more open environment had either a neutral or a positive impact on the creation of new mouse varieties.

Finally, we consider the impact of the openness shocks on the diversity in mice creation. To this effect, we divide the set of mice created through a particular technology in a given year from each citation year into two mutually exclusive types: those published in journals which had previously published mice associated with that same technology (“Old Creation Journals”), and those which have not previously published any such mice (“New Creation Journals”). Our specifications here are simply a two-equation version of the specifications employed in (9-1); we estimate the impact of each of the NIH agreements on new mice published in old versus new journals, accounting for fixed differences across each technology type, calendar year effects, and including time trends as above. The results are in (9-3) and (9-4). We find economically and statistically significant (positive) differences between the number of “new journals” and “old journals” in which new mouse-articles are published for the window periods of both Cre-lox and Onco mice, and during the treatment period for Onco mice.

Overall, the results in Table 9 show no significant reduction in the creation of genetically modified mice following the NIH agreements, but instead a strong increase in the diversity of mouse creation, as reflected by the new journals in which mouse-articles are published. Under the traditional linear view of sequential innovation, new research lines can only result from the introduction of new tools (in this case, new mice), and greater upstream openness reduces the incentives for their creation. By contrast, our findings are more consistent with a setting where new lines can also result from cross-fertilization with existing lines by scientists engaging in exploratory research, and where this cross-fertilization is itself facilitated by openness.

Table 1.9: Impact of Openness on Overall Mouse Creation and Mouse Creation in New vs. Old Journals

	[Incident rate ratios reported in square brackets] Estimated coefficients in 2 nd line. (Block bootstrapped SEs reported in parentheses)					
	NEGATIVE BINOMIAL		STACKED NEGATIVE BINOMIAL			
	(9-1) DV= Annual Mouse Creation	(9-2) DV= Annual Mouse Creation	(9-3a) DV= New Creation Journals	(9-3b) DV= Old Creation Journals	(9-4a) DV= New Creation Journals	(9-4b) DV= Old Creation Journals
Cre-lox-Window	[1.657]*** 0.505 (0.191)	[1.318] 0.276 (0.280)	[3.629]*** 1.289 (0.316)	[0.767]* -0.265 (0.135)	[3.367]*** 1.214 (0.396)	[0.797] -0.227 (0.422)
Post-Cre-lox	[1.545]* 0.435 (0.252)	[1.208] 0.189 (0.296)	[1.763] 0.567 (0.449)	[1.271]*** 0.240 (0.076)	[1.701] 0.531 (0.531)	[1.276] 0.244 (0.577)
Onco-Window	[1.579]* 0.457 (0.248)	[1.031] 0.031 (0.097)	[1.556]*** 0.442 (0.095)	[1.007] 0.007 (0.062)	[1.075] 0.072 (0.292)	[0.756] -0.280 (0.299)
Post-Onco	[1.835]*** 0.607 (0.113)	[0.972] -0.028 (0.182)	[2.586]*** 0.950 (0.125)	[1.256]*** 0.228 (0.077)	[1.462] 0.380 (0.429)	[0.841] -0.173 (0.440)
CONTROL VARIABLES						
Age Controls	Yes ⁺	Yes ⁺	Yes ⁺	Yes ⁺	Yes ⁺	Yes ⁺
Year FEs	Yes	Yes	Yes ⁺⁺		Yes ⁺⁺	
Technology FEs	Yes	Yes	Yes		Yes	
Log-likelihood	-287.449	-518.927	-335.559		-580.809	
# of Technologies	4	8	4		8	
# of Observations	78	136	156		272	

Significance levels: * 10% ** 5% *** 1%

+ Age controls include a cubic polynomial in the age of the technology, plus a linear correction in age for the much-older spontaneous technology. Note that there are separate effects for the two margins in stacked regressions.

++ Calendar year fixed effects include a set of indicator variables common to both margins in a given regression, and a linear difference variable which allows for a constant difference in growth rates between the two margins.

Tests of Differences Between Coefficients:

(9-3): $\beta(\text{Cre-lox-Window effect on New Creation Journals}) - \beta(\text{Cre-lox-Window effect on Old Creation Journals})$:

Estimate = 1.554; SE = 0.451; Prob>|z| = 0.001

 $\beta(\text{Onco-Window effect on New Creation Journals}) - \beta(\text{Onco-Window effect on Old Creation Journals})$:

Estimate = 0.435; SE = 0.050; Prob>|z| < 0.001

 $\beta(\text{Post-Onco effect on New Creation Journals}) - \beta(\text{Post-Onco effect on Old Creation Journals})$:

Estimate = 0.722; SE = 0.078; Prob>|z| < 0.001

(9-4): $\beta(\text{Cre-lox-Window effect on New Creation Journals}) - \beta(\text{Cre-lox-Window effect on Old Creation Journals})$:

Estimate = 1.442; SE = 0.195; Prob>|z| < 0.001

 $\beta(\text{Onco-Window effect on New Creation Journals}) - \beta(\text{Onco-Window effect on Old Creation Journals})$:

Estimate = 0.352; SE = 0.140; Prob>|z| = 0.012

 $\beta(\text{Post-Onco effect on New Creation Journals}) - \beta(\text{Post-Onco effect on Old Creation Journals})$:

Estimate = 0.553; SE = 0.241; Prob>|z| = 0.022

1.7 Conclusion

In this paper we argued that greater openness of early-stage research leads to an increase in the diversity and the exploratory nature of follow-on innovation. Decreasing costs of access to pre-existing innovations has a differentially stronger impact on speculative research, increasing the likelihood of establishing entirely new research directions. This increase in the scope of follow-on research provides additional incentives for new early-stage projects, offsetting the potential drawbacks of reduced reach-through rights. Further, academic research can be motivated by the desire for recognition and citation within the academic community, in addition to the incentives provided by intellectual property. We tested our hypotheses by examining a natural experiment in openness within the academic community: NIH agreements during the late 1990s that circumscribed IP restrictions for academics and increased the openness of key types of genetically engineered mice and the research tools associated with their production. Our empirical results suggested that the NIH agreements had a profound and long-lasting impact on follow-on research. Moreover, we found that these openness shocks not only increased the overall flow of research using specific engineered research mice, but also expanded both the diversity of researchers working on particular research lines, and the diversity of the research lines being pursued. Finally, we found that increased openness did not result in a reduction in the flow of new mouse creation. Our results therefore highlight a key limitation in the current literature on intellectual property and innovation: the impediments intellectual property restrictions may place on the diversity of follow-on research, particularly in the case of academic researchers seeking to explore the potential uses of multi-purpose research tools.

The analysis developed in this paper could be extended in several interesting directions. One avenue would be to reassess the Bayh-Dole Act based on our findings. Indeed, our results highlight one of the possible dangers of excessive IP enforcement: namely, if IP is used to restrict openness at very early stages of the research line, then it may stifle exploratory projects that are necessary for diverse follow-on innovation. Importantly, an attempt to use IP protections to ensure strong incentives for early-stage research may prove counter-productive, as the lack of exploration may severely limit both the scope and total value of follow-on research stemming from the initial innovation. Further, our analysis

suggests that more attention be paid by economists to recent corporate attempts to generate new sources of profit through building on the openness of knowledge production by others. Tapscott and Williams (2006) explain how IBM has recovered from competition with Microsoft by engaging in the openness promoted by the Linux community. By contrast, the experience of DuPont and other companies that continued to enforce patents while also attempting to engage with the open scientific community was less successful (Huang and Murray, 2008). This pattern suggests the need for a systematic analysis of the forces and trade-offs at work in an economic environment where both proprietary-technology and open-technology firms compete with each other and cooperate with open communities, setting the stage for future research.

Chapter 2

Strategic Corporate Layoffs

2.1 Introduction

Voluntary disclosures of bad news by firms are often immediately followed by similar disclosures by other firms. For instance, in early 2010, after sudden acceleration in some vehicles, Toyota recalled 5.6 million vehicles in the United States. Within weeks, Honda, Nissan, Suzuki and GM quickly followed with recall announcements of their own. Such disclosure dynamics are also observed in the release of various other corporate news (e.g. for earnings announcements and write-downs), and are documented in several studies¹. This paper is the first to document a similar phenomenon in the announcements of layoffs by firms. While announcing a layoff is operationally similar to disclosures of other bad news, it potentially has larger welfare implications since the timing of layoffs is tightly linked to unemployment dynamics. In this paper we investigate the factors that lead to the clustering of layoff announcements, and study the welfare implications of such mechanisms.

To gauge at the degree of clustering in layoff announcements, consider the behavior of the top 3 US firms in the banking industry (Bank of America, J.P. Morgan and Citi-

¹Acharya et. al (2009) present a model based on asymmetric information that rationalizes this behavior. Also, Dellavigna and Pollet (2008) provide evidence on the Friday effect and argue strategic disclosure of bad news may arise from manager's exploiting time-varying attention span of the market. Lastly, Tse and Tucker (2010) provide evidence for clustering of bad news in the form of earnings warnings by firms within an industry.

group) and the automobile manufacturing industry (G.M., Ford and Chrysler) around the 2001 recession. Figure 2.1 represents (superimposed) timelines of layoff announcements for each of the 6 firms from 2000 to 2003. From the figure it is evident that the layoff announcements tend to be clustered within the industry. In many cases we observe layoff announcements clustered within the same week (for e.g. in the Fall of 2002, Chrysler and Ford announced layoffs on the same day). In the same figure it is also evident that there are frequent cases of clustering of announcements across industries. For e.g. in the Fall of 2003, J.P. Morgan and G.M. announced their layoffs in the same week, and days later Bank of America announced a layoff. A more systematic examination of layoff announcement behavior (Section 6) reveals that such clustering behavior in layoff announcements is observed only in publicly-traded firms (“public” firms), and not in *similar* privately-held firms (“private” firms). These facts motivate the central question in this paper: why do public firms engage in clustering of layoffs, while private firms don’t; and what are the aggregate implications of such behavior?

There are various theories that predict clustering of layoff announcements (e.g. common shocks), and a central contribution of this paper is to evaluate the empirical strength of the various leading theories. We interpret the observed degree of clustering and differences between public and private firms through a model based on asymmetric information between manager of firms and the (financial) market. The central mechanism of the model is as follows. The market perceives a layoff announcement as a negative signal about the manager’s ability. When the aggregate business conditions are adverse (e.g. during recessions or industry-wide downturns), however, the market will attribute greater blame to external factors than to managerial ability. This generates incentives for the managers to time their layoff announcements to appear during bad times, thereby minimizing the blame for the bad news. This key idea of our paper is a counterpart to the early paper by Gibbons and Katz (1991), who provide evidence that laid off workers are viewed less favorably by the market than are those losing jobs in plant closings. We invoke the same Gibbons-Katz mechanism to illustrate that managers that lay off in recessions will bear a lower reputation penalty, which will lead to clustering of layoff announcements.

The model has two main cross-sectional predictions. First, if managers care more about their reputation (relative to the cash flows of the firm), then they are more likely to engage

in layoffs during adverse macro states. Analogously, the model has the same prediction for firms with managers who don't have a long-track record, since the market's beliefs about their ability are more sensitive to any new information. Our model is a three-period static model, and for the purposes of empirical applications we map it to both daily frequency tests and business cycle (annual) frequency tests. Since clustering of layoff announcements happen within days of one another the daily frequency data is suited to test for such clustering behavior. Nevertheless, since we are also interested in estimating the welfare-relevant impact of reputation management, we study layoff behavior over the business cycle using annual data.

To test the various predictions of the model we rely on two novel datasets. The first dataset consists of layoff announcements by the largest publicly-listed firms (Fortune 500 constituents) and largest privately-held firms (Forbes 100 constituents), collected from daily issues of the Wall Street Journal between 1970 and 2010. Since we are also interested in potential welfare consequences of the clustering mechanism, we supplement our analysis of layoff announcements with a second dataset of actual mass layoffs. This consists of confidential microdata of actual mass layoffs from the Bureau of Labor Statistics under their Mass Layoff Statistics program. We are the first outside researchers that were granted permission to use this microdata to analyze the predictions of our model. Apart from the novelty, one benefit of this dataset is that since it collects data from unemployment insurance (UI) claims, it allows us to observe the timing and the exact number of displacements arising from mass layoffs. Consequently, our analysis that uses this BLS data is less subject to biases in reporting, or underreporting of information that may affect our other dataset based on reporting by the Wall Street Journal.

At the business cycle frequency we rely on differences in public and private firm behavior to estimate the impact of reputation management on layoff propensity. Why should managers in private firms have less incentive to manage their reputation? Public firms differ from private firms along three major margins, all of which make their managers more likely to manage their reputation in financial markets, relative to a similar private firm. First, since public firms sell shares to outside investors who are not involved in managing the firm, there exists separation between ownership and control. This may lead to agency problems if managers' interests diverge from those of their investors (Jensen and Meckling

(1976)). Second, owners of public firms typically have shorter horizons, since liquidity makes it easy for shareholders to sell their stock at the first sign of trouble rather than actively monitoring management. This relative myopic behavior of investors, weakens incentives for effective corporate governance (Bhide (1993)), and generates incentives for managers to be myopic in their reputation management (Stein (1989)). Third, managers of public firms are subject to takeover threats, which are, in part, dependent on the stock price of targeted firms. This can lead to managerial myopia in public firms in order to actively manage current stock prices (Stein (1988) and Edmans et. al (2009))². Therefore, if reputation management drives layoff behavior over the business cycle, the first-order effect should show up when comparing differences in behavior of *similar* public and private firms. Here we emphasize reputation management in *financial markets*, since the managers of both public and private firms may have similar motivations for reputation management among other constituents. Based on a matching estimator that matches each public firm to a private firm based on size and 4-digit industry level, we find that the propensity to layoff of private firms increases by roughly 2.4 percentage points in recession months. By contrast, public firms' propensity to layoff in recessions increases by an additional 2.4-3.2 percentage points. That is, public firms are twice as likely to engage in a mass layoff in a recession month compared to their matched private counterpart. In a range of tests we show that these differences are not being driven by public-private differences in lifecycle effects, leverage, size of workforce, or on our matching criteria. Within our sample of public firms, we find that firms that are predicted to be more strategic are also the ones which are more likely to engage in a mass layoff during recessions. Our results, therefore, suggest that the difference in reputation management is an important driver of the observed differences in the cyclicalities of layoffs between public and private firms.

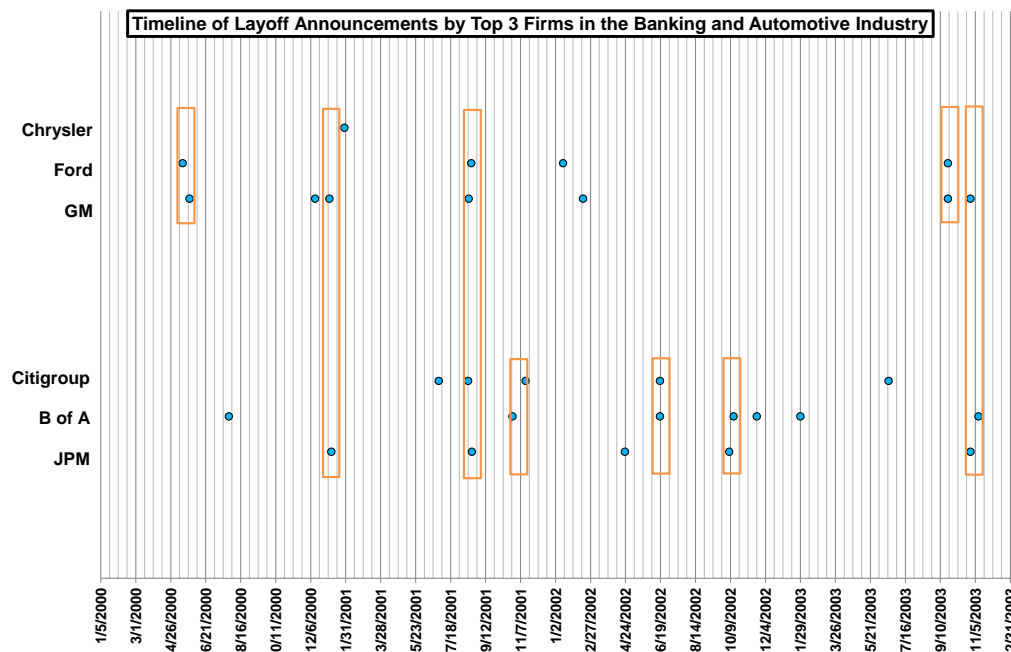
At the daily frequency, we also find support for our model. We show that a large firm announcement (i.e. largest 20 firms based on past year's revenue) is associated with future

²Private firms, in contrast, are often owner-managed and even when not, are both illiquid and typically have highly concentrated ownership, which encourages their owners to monitor management more closely. Indeed, evidence from the Federal Reserve's 2003 Survey of Small Business Finances (SSBF) shows that 94.1% of the larger private firms in the survey have fewer than ten shareholders (most have fewer than three), and 83.2% are managed by the controlling shareholder. As a result, agency problems are likely to be greater among public firms than among private ones.

layoffs by other Fortune 500 firms, but not with past layoffs. We find that this effect is twice as strong if the largest 20 firm is in the same industry as the follower firm. For our sample of privately held firms we find no such clustering behavior either before or after the large firm layoff announcement. Moreover, when we compare the characteristics of firms that lay off within 5 days after a large firm announcement (“followers”) to those that layoff within 5 days before a large firm announcement (“counterfactual followers”), we find that the follower firms have a greater likelihood to be managed by short-tenured CEOs (i.e. with a tenure between 0 and 4 years) and the follower firms place greater reliance on equity-linked compensation for their CEOs. Both these characteristics are linked to reputation management behavior, and suggest that reputation management is an important driver for the timing of layoff announcements at high frequencies. Lastly, to investigate whether such high frequency of clustering is associated with welfare-relevant outcomes we track the followers and counterfactual follower firms’ behavior at business cycle frequencies. We find that the follower firms are roughly 3 percentage points more likely to engage in a mass layoff in recession month compared to the counterfactual follower firms. This link between the daily frequency reputation management behavior to cyclicalities of layoffs over the business cycle is strong evidence in support of the reputation management hypothesis of this paper.

Figure 2.1: Timeline of Layoff Announcements

Figure 2.1: Timeline of layoff announcements by top 3 firms in the banking and automotive industry between 2001 and 2003. A dot represents a layoff announcement on a given day. To highlight clustering of announcements we include (orange) boxes with width 30 days whenever multiple announcements occur within a period of 30 days. The longer boxes represent clustering across industries, while shorter boxes represent clustering within industries.



2.2 Statistical Evidence of Clustering

As illustrated in the case study, we observe that layoff announcements are often clustered within days of each other. The size of these clusters range from within a day to over two weeks. Though this is suggestive evidence of excess clustering, it is not clear whether this observation represents a general trend which applies to other periods and other firms in the economy. In this section, we take a systematic approach to identify and characterize the nature of excess clustering in firms' layoff announcements.

Our approach uses a measure called the scan statistic, which is used to detect unusual clusters in a sequence of events that occur over time. The approach is known as "moving window analysis" in the engineering literature (see Glaz et. al (2003)). Given N

events independently drawn from the uniform distribution on $[0, 1)$, S_w denotes the largest number of events to be found in any subinterval of $[0, 1)$ of length w . That is, a small window $[t, t + w]$ of fixed size is moved along the interval, calculating the total number of events in each subinterval. The maximum of these local statistics (calculated for each of the subintervals) is called the scan statistic, S_w . Under the null hypothesis of a uniform distribution of events, H_0 , the approach entails specification of a critical value c_α , such that $\Pr[S_w > c_\alpha] = \alpha$. If the maximum observed local statistic is larger than or equal to c_α , then we should reject the null hypothesis and infer existence of non-homogeneity: a local region with a statistically significant cluster.

The distribution of scan statistic is a function of two parameters: the size of the sub-window, w (relative to the size of the entire interval); and the number of events, N , which occur in the entire interval $[0, T]$. We denote the p-value of this test as $\Pr[k; N, w]$. This p-value should be interpreted as follows: *under the null of N events independently drawn from the uniform distribution on $[0, 1)$, $\Pr[k; N, w]$ is the probability that we observe k or more events in any subwindow of size w .*³

The unit of time for our tests is business days. We conduct our tests for two different interval sizes: 60 business days (approximately one quarter) and 20 business days (approximately one month). Our sample period begins in 1970 and ends in 2010, and we run separate tests for every non-overlapping window in this period of 41 years. Since the test has low power when N is small, we exclude months in which we observe fewer than 5 layoff announcements. Also, we run the tests for two categories: all industries combined; and the manufacturing industry. This allows us to assess whether there is excess clustering at both the aggregate and the industry level.

³Under the null hypothesis, the probability of observing a scan statistic, S_w , greater than k , can be characterized as a function of the two parameters:

$$\Pr[S_w \geq k] \equiv \Pr[k; N, w]$$

Exact estimates of this common probability exists for certain cases, and researchers have to rely on approximations for the other cases.

Figure 2.2: Scan Statistic Results

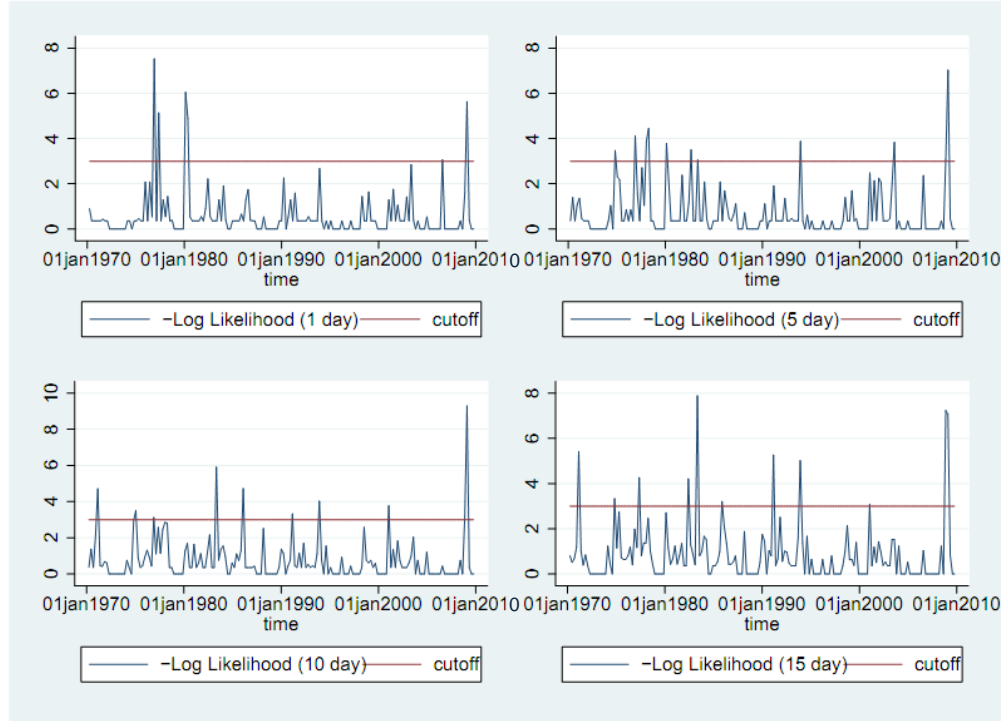


Figure 2.2: This graph reports the Log p-values (multiplied by negative unity) of the sequence of tests for each non-overlapping 60 (business) day window to identify clustering at 1, 5, 10 or 15 day horizons when the null is that there is layoff announcements are distributed uniformly over each 60 day window. The horizontal red line corresponds to a significance level of 0.05. Correspondingly, whenever p-values exceed the red line, we can reject the null hypothesis at the 5% level.

Figure 2.2 plots the results of our analysis for each non-overlapping interval of 60 business-days. To facilitate viewing, we present the negative log of the p-value as our y-axis variable. Therefore, a higher value suggests that we can reject the null with greater confidence. Though this sequence of individual tests is suggestive of several episodes of clustering, we would like to combine the results from these different independent observations into a single statistical test. For this, we rely on Fisher's method to combine the p-values from our tests into a single statistic, using the formula $X^2 = -2 \sum_{i=1}^k \log(p_i)$, where p_i is the p-value for the i-th independent test. This 'combined p-value' is reported in

the last column of Appendix Table 1. We find that we can reject the null of no excess clustering for subwindows of 5 or more days using one-month intervals, and for subwindows as small as three days using quarterly intervals. Having established the existence of excess clustering, we proceed to offer a potential explanation for this phenomenon in Section 3, where we present our theoretical model.

2.3 Theoretical Model

In this section, we present a reputation-based model of management layoff decisions, focusing on the tradeoff between firm profits and the perception of managerial talent. A similar model was presented in Rajan (1994), in which he studies the clustering of credit policies by banks. We focus on a two-period version of the model in the main text, incorporating fully rational Bayesian expectations and solving for the set of trembling-hand perfect equilibria. We discuss the implications of relaxing these assumptions and expand to multiple periods in the theoretical appendix.

2.3.1 Model Setup

Our model starts at date 0, and ends at date 3. There is no discounting between periods. There are two types of agents in this model: firm managers and the market.⁴ Managers care about the profits of their firm, and about their reputation with the market. The market takes no direct action in this model, but simply observes the actions of managers, and updates its priors according to Bayes' Rule.

Firm managers are the primary decision-making agents in this model. Each manager i is associated with a firm which, at date 0, begins a new project and hires one unit of labor to engage in production. There is continuum of manager types, which differ along the dimension of managerial talent, denoted by η_i . The only restriction on the distribution

⁴There are many possible interpretations for the role of the market. One possibility is the population of equity market investors - this interpretation links our model to concerns about stock price responses to layoffs. Another option is the demand side of the market for managerial talent - this interpretation is more in line with the literature on career concerns. We do not pin down a specific interpretation in order to allow for the broadest possible application of the model.

of talent, described by density $f(\eta_i)$, is that its support be within the unit interval, $[0,1]$. For convenience, we define the mean and variance of the distribution of talent to be μ and σ_η^2 , respectively.

After the project is undertaken, the aggregate economic state is realized at date 1.⁵ The aggregate state is denoted by $s \in S = \{N, A\}$: it can be adverse (A) with probability π , or normal (N) with probability $1 - \pi$.

The probability that a project succeeds depends on both the talent of the manager and the aggregate state, and is given by:

$$\theta_s^i = \eta_i \lambda_s \quad (2.1)$$

such that $\lambda_N = 1$, and $\lambda_A = 1 - \delta$. In the adverse state the probability of project success is $\eta_i(1 - \delta)$, and in normal states it is η_i . There is symmetric uncertainty about the aggregate state for both managers and the market throughout all time periods.

The manager privately observes the outcome of his firm's project at date 1. If the project was successful, there is no decision to make: the project continues into date 2, where it generates earnings of \$1, and then ends at date 3. If the project is not successful, the manager has to decide whether to terminate or continue the project. Termination involves firing the labor force hired at time 1, and is therefore fully observable. The firm's date 2 earnings are zero if it terminates an unsuccessful project at date 1. We label this approach as the “*terminate*” policy.

Instead of termination, the firm can hide the unsuccessful outcome of the project from the market by not laying off the labor force assigned to the project. If the manager adopts such a policy, he must also match the \$1 of earnings that would be created at date 2 by a successful project. In doing so, he incurs a private reduction of $1 + C$ in earnings at date 3, which is not observed by anyone else. Relative to the *terminate* policy, this decision delays the end of the project by one period. We therefore label this approach as the “*delay*” policy.

We assume that adopting the *delay* policy is costly relative to the decision to *terminate*. This is given by *Assumption 1*: $C > 0$. This assumption implies that in a first-best world, the *delay* policy should not be adopted.

⁵We can interpret the aggregate economic state as either an economy-wide indicator, or a measure of the health of a particular sector.

Despite its inefficiency, managers have an incentive to adopt the *delay* policy because it is better for their reputation to hide an unsuccessful project outcome. In setting up the maximization problem for managers, let $D \in 0, 1$ represent whether or not a given manager adopts the *delay* policy when his firm's project fails. We can then describe managers' preferences by the following utility function:

$$\max_D U_i = -DC + \gamma E_{mkt} [\eta_i | \hat{D}, \text{Layoffs}] \quad (2.2)$$

such that γ is the utility weight the management places on his reputation in the eyes of the market, and \hat{D} is the conjecture of the manager's strategy that the market uses to interpret the observation of layoffs or no layoffs.

2.3.2 Reputation and Updating Rules

The market's updating rule depends on 2 factors: a) its conjecture about the manager's strategy in addressing a failed project; and b) whether or not it observes layoffs. We begin our equilibrium analysis by focusing on four primary cases:

Conjecture:	No Layoffs		Layoffs	
'Terminate'	E_{mkt}	$\eta_i \hat{D} = 0, \text{Layoffs} = 0$	E_{mkt}	$\eta_i \hat{D} = 0, \text{Layoffs} = 1$
'Delay'	E_{mkt}	$\eta_i \hat{D} = 1, \text{Layoffs} = 0$	E_{mkt}	$\eta_i \hat{D} = 1, \text{Layoffs} = 1$

When the market conjectures that the firm will adopt the *Terminate* policy

If the market believes the firm is going to adopt the *terminate* policy, i.e. $\hat{D} = 0$, then the firm will lay off the project's workers upon failure, and not lay off workers when the project succeeds. Therefore, not observing layoffs implies that the firm's project has succeeded. This makes the updating rule straightforward: the observation of layoffs or no layoffs is perfectly correlated with the outcome of the project.

Using Bayes Rule we can calculate the resulting posteriors as:

$$\begin{aligned} E_{mkt} [\eta_i | \hat{D} = 0, \text{Layoffs} = 0] &= E_{mkt} [\eta_i | \text{Project Succeeds}] = \mu + \frac{\sigma_\eta^2}{\mu} \\ E_{mkt} [\eta_i | \hat{D} = 0, \text{Layoffs} = 1] &= E_{mkt} [\eta_i | \text{Project Fails}] = \mu - \frac{\sigma_\eta^2 (1 - \pi\delta)}{1 - \mu (1 - \pi\delta)} \end{aligned} \quad (2.3)$$

(Proof in Appendix.)

When the market conjectures that the firm will adopt the *Delay* policy

We get an analogous updating rule for the case in which the market believes the firm is going to adopt the *delay* policy, i.e. $\hat{D} = 1$. Under perfect play, the outcome of layoffs under this policy will never occur. We therefore introduce trembles and focus on trembling-hand-perfection as our equilibrium concept. Because successful projects continue automatically, they do not require any action from the manager and are not susceptible to trembles. By contrast, the decision to adopt the *delay* policy requires a direct action by the manager, who could tremble and choose to terminate the project instead. Therefore, Whenever the firm engages in layoffs the market knows the project must have failed, even though this outcome will (almost) never be observed in equilibrium. The updating rule in this situation can be calculated as follows:

$$E_{mkt} \left[\eta_i | \hat{D} = 1, \text{Layoffs} = 1 \right] = E_{mkt} [\eta_i | \text{Project Fails}] = \mu - \frac{\sigma_\eta^2 (1 - \pi\delta)}{1 - \mu (1 - \pi\delta)} \quad (2.4)$$

By contrast, when the market observes no layoffs, they do not know whether the project failed or not. This is because the firm is expected to adopt the *delay* policy of no layoffs irrespective of project outcomes. Therefore, when the market observes no layoffs, they get no new information about the firm, and the updating rule is simply:

$$E_{mkt} \left[\eta_i | \hat{D} = 1, \text{Layoffs} = 0 \right] = E_0 [\eta_i] = \mu \quad (2.5)$$

(Proof in Appendix.)

2.3.3 Equilibrium Selection

In equilibrium the market conjecture about the manager's policy must be correct, and hence $\hat{D} = D_i$.

To support the equilibrium where the manager always adopts the *terminate* policy, the following incentive compatibility condition must hold:

$$\gamma E_{mkt} \left[\eta_i | \hat{D} = 0, \text{Layoffs} = 1 \right] \geq -C + \gamma E_{mkt} \left[\eta_i | \hat{D} = 0, \text{Layoffs} = 0 \right] \quad (2.6)$$

By contrast, to support the equilibrium where the manager always adopts the *delay* policy, the IC constraint is:

$$\gamma E_{mkt} [\eta_i | \hat{D} = 1, \text{Layoffs} = 1] \leq -C + \gamma E_{mkt} [\eta_i | \hat{D} = 1, \text{Layoffs} = 0] \quad (2.7)$$

Using the Bayesian analysis in the previous section, the above constraints, respectively, simplify to:

$$C \geq \gamma \sigma_\eta^2 \left[\frac{1}{\mu} + \frac{1 - \pi\delta}{1 - \mu(1 - \pi\delta)} \right] \quad (2.8)$$

and

$$C \leq \gamma \sigma_\eta^2 \left[\frac{1 - \pi\delta}{1 - \mu(1 - \pi\delta)} \right] \quad (2.9)$$

From the above constraints, it is clear that for sufficiently high values of γ and σ_η^2 , managers will choose to always adopt the *delay* policy. At the same time, for sufficiently low values of these variables, managers will always choose to adopt the *terminate* policy. Having already characterized these equilibria, we now move to consider the intermediate set of parameter values, which support neither pure-strategy equilibrium.

For intermediate values of γ and σ_η^2 , the equilibrium reputation penalty under the *terminate* policy is so large that managers prefer the *delay* policy, and the equilibrium reputation penalty under the *delay* policy is so small that they prefer the *terminate* policy. This means that for these parameter values, there is no equilibrium in pure strategies. We therefore proceed to analyze a mixed-strategy equilibrium, where managers randomize between adopting the *terminate* and *delay* policies.

As in the previous section, we begin our characterization of the mixed-strategy equilibrium by focusing on the market's posterior following an observation of either layoffs or no layoffs. In this case, instead of a binary conjecture about the policy of the manager, we move to a continuous conjecture $\hat{D} \in (0, 1)$ which corresponds to the probability with which the market expects the manager to choose the *delay* policy, conditional on project failure. In such a setting, we calculate the market's posteriors as:

$$E_{mkt} [\eta_i | \hat{D}, \text{Layoffs} = 0] = \mu + \frac{(1 - \hat{D})(1 - \pi\delta)\sigma_\eta^2}{\hat{D} + (1 - \hat{D})(1 - \pi\delta)\mu} \quad (2.10)$$

and

$$E_{mkt} [\eta_i | \hat{D}, \text{Layoffs} = 1] = \mu - \frac{\sigma_\eta^2(1 - \pi\delta)}{1 - \mu(1 - \pi\delta)}$$

(Proof in Appendix.)

Note that the above posteriors match up with pure-strategy beliefs when we take the limits as $\hat{D} \rightarrow 0$ and $\hat{D} \rightarrow 1$ for the cases of the *terminate* and *delay* policies, respectively.

To complete the characterization of the mixed-strategy equilibrium, we need the manager to be indifferent between the two strategies available to him. Using the same IC constraint framework as in the pure-strategy case, we need:

$$\gamma E_{mkt} [\eta_i | \hat{D}, \text{Layoffs} = 1] = -C + \gamma E_{mkt} [\eta_i | \hat{D}, \text{Layoffs} = 0] \quad (2.11)$$

Using the posterior market beliefs for the mixed-strategy case, the above constraint simplifies to:

$$C = \gamma \sigma_\eta^2 \left[\frac{(1 - \hat{D})(1 - \pi\delta)}{\hat{D} + (1 - \hat{D})(1 - \pi\delta)\mu} + \frac{1 - \pi\delta}{1 - \mu(1 - \pi\delta)} \right] \quad (2.12)$$

The equation above allows us to solve for the manager's randomization probabilities in the mixed-strategy equilibrium. Note that because the RHS is monotonically decreasing in \hat{D} , there is a unique set of mixing probabilities that supports equilibrium play for any given set of parameter values. Further, because the limits of the expression match up to the pure-strategy equilibria described in the previous section, there is a continuous progression from always choosing the *terminate* policy, through a mix of both options, and finally to always choosing the *delay* policy, as the product of γ and σ_η^2 increases from zero.

2.3.4 Equilibrium Implications

Based on the characterization of the model's equilibria in the previous section, we conclude that the firm has an incentive to undertake the *delay* policy when:

- the market thinks the aggregate state is likely to be normal (represented by a low value of π)
- the manager places a high weight on reputation (as measured by γ)
- layoffs are particularly informative about the manager's ability, due to significant uncertainty in the market's prior beliefs (i.e. a high value of σ_η^2).

The above implications have direct links to observable variables in empirical corporate finance. A high value of γ is likely to be associated with firms that incentivize their management with high-powered, market-based compensation packages. As for the informativeness of layoffs, a high value of σ_η^2 (a sufficient statistic for the signal-to-noise ratio in our model) is likely to be associated with firms that have a new management, precisely because the market will have less information about them, and any action taken by them will be relatively more informative.

These conclusions describe conditions under which the firm will undertake the *delay* policy, despite the inefficient reduction in earnings that result from it. To gain an insight into this central result, Figure 2.3 plots the equilibrium policies of managers based on their values of γ (degree of reputational concerns) and σ_η^2 (variance of market's prior about firm). Managers who adopt the *delay* policy will lie above and to the right of mixed-strategy region. In this region, managers and their firms will delay project termination and avoid layoffs, despite their project failing.

Figure 2.3: Equilibrium Layoff Policy

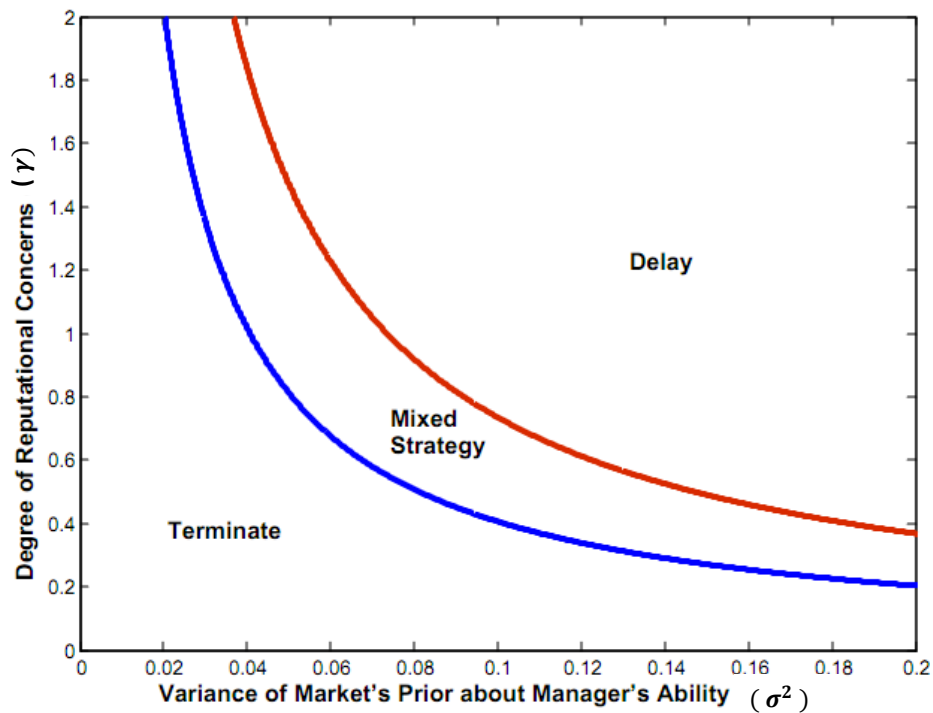


Figure 2.3: *Parameter Space and Equilibrium Layoff Policy.* The region on the right of the solid lines represents the parameter space in which the equilibrium layoff policy is to delay. Analogously, the equilibrium layoff policy in the region on the left is to terminate. The region between the two lines represents the parameter space in which the equilibrium policy is a mixed strategy, which randomizes between terminate and delay.

An interesting implication of the model is the effect of changing beliefs about the aggregate state S , where the expectation of an adverse state is measured by π . Figure 2.4 plots the same boundaries as Figure 2.3, and adds another set of boundaries to demonstrate the

effect of the market's perception about aggregate state becoming pessimistic. Assuming that this perception is justified, there will be a direct effect of fewer successful projects in an adverse economic environment. Because of this, the market is less likely to attribute the negative signal of a layoff to the manager's level of talent, and consequently the reputational concern associated with layoffs diminishes. This is illustrated by the rightward shift of the boundaries in Figure 2.4. As a result, the parameter region for which firms will adopt the *delay* policy shrinks. More firms now will choose to announce layoffs if their project fails.

Figure 2.4: Impact of Aggregate State Deterioration

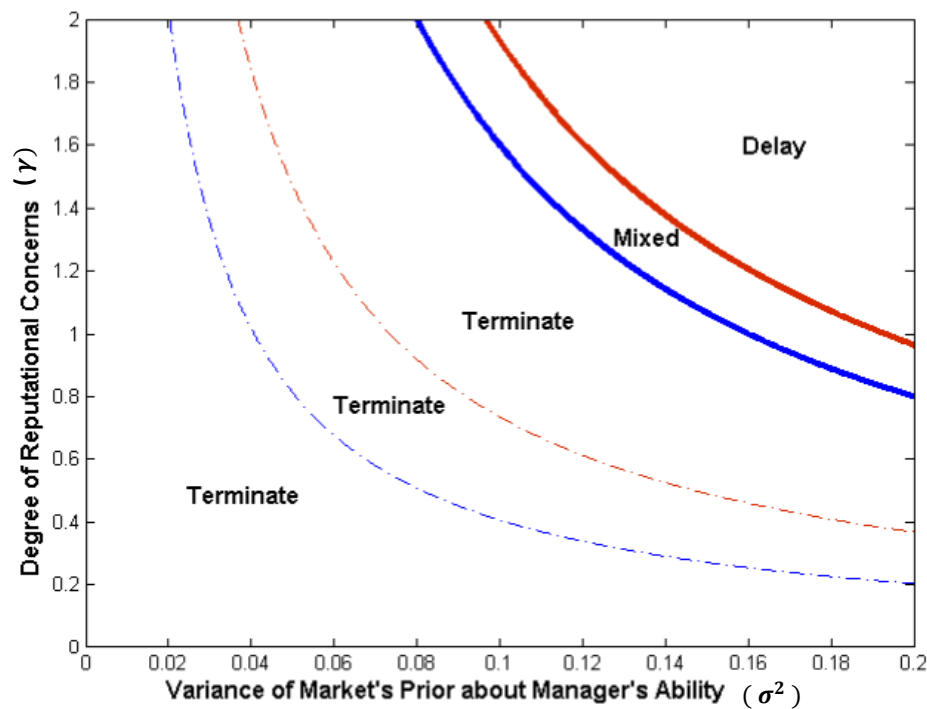


Figure 2.4: Comparative statics – when belief about aggregate state worsens. The dotted lines represent the mapping of different regions of the parameter space to different equilibrium layoff policy before we take comparative statics. When belief about aggregate state worsens, the lines separating the regions shift rightward, and are then represented by the solid lines.

In addition to the direct effect of adverse economic conditions leading to higher rates of project failure, our model also predicts a shift-in-equilibrium effect: conditional on project failure, a larger fraction of managers will choose to terminate their projects and engage in layoffs during these economic downturns.⁶ In the pure-strategy regions of the parameter space, there is no shift in equilibrium because managers are effectively at a corner solution. Those who strictly prefer the *delay* policy will continue to have a layoff rate of precisely zero, while those who strictly prefer the *terminate* policy will have a layoff rate equal to their project failure rate: $1 - \eta(1 - \pi\delta)$, or $1 - \mu(1 - \pi\delta)$ on average. The interesting case is that of managers in the mixed-strategy region. We describe their layoff rate in the following proposition:⁷

Proposition 2.1. *For managers in the mixed-strategy region of the parameter space, an increase in the expected probability of a downturn leads to a strictly higher rate of layoffs. Specifically:*

$$\frac{\partial \Pr[\text{Layoffs} \mid \pi, \gamma, \sigma_\eta^2; C, \delta]}{\partial \pi} = \frac{\delta \gamma \sigma_\eta^2}{C [1 - \mu(1 - \pi\delta)]^2} > 0$$

Proof. See appendix. □

From this result, it follows that managers who care most about reputation (i.e. have a high value of γ) and who have a short track record (i.e. a high value of σ_η^2) are most likely to be affected by the adverse shift in market's perception of the aggregate state. These insights are summarized in the following two corollaries:

Corollary 2.1. *If the market's belief about a firm's management is less precise, then the manager is more likely to announce layoffs in downturns. Specifically:*

$$\frac{\partial^2 \Pr [\text{Layoff} \mid \pi, \gamma, \sigma_\eta^2; C, \delta]}{\partial \pi \partial \sigma_\eta^2} \geq 0$$

⁶In the multi-period model discussed in the appendix, we show that firms with failed projects are likely to continue them until the next economic downturn, effectively saving their layoffs until they can implement them without suffering the normal reputational penalty.

⁷While Proposition 1 and the two corollaries below and the rest of the analysis focus on changes in the probability of experiencing an adverse aggregate state, similar results obtain when considering an increase in the severity of the adverse state, represented by the magnitude of δ .

Corollary 2.2. *If the manager's utility function puts more weight on his reputation, then the manager is more likely to announce layoffs in downturns. Specifically:*

$$\frac{\partial^2 \Pr [\text{Layoff} \mid \pi, \gamma, \sigma_\eta^2; C, \delta]}{\partial \pi \partial \gamma} \geq 0$$

2.3.5 Extending the Model: Impact of a Large Firm

In this section we extend the model presented above by introducing a large firm into the economy. The notion of large here is that the firm's performance contains information about the aggregate state of the economy. By contrast, the performance of small firms is heavily influenced by conditions in their sector and their local market, so the ability to obtain information about the aggregate state from the performance of a small firm is assumed to be negligible. As a result, the large firm's layoffs decision will influence the other firms in the economy through the information it provides about the aggregate state. As shown in Propositions 1 and 2, the market's beliefs about the aggregate state have a strong influence on firms' layoff decisions. With the addition of a large firm, the model therefore generates strategic interaction between firms.

In formulating this extension of the model, we move from a general-purpose metric of beliefs about the aggregate state, π , to a firm-specific metric represented by π_i . As before, π_i represents the probability of an adverse economic state; however, in this extension, it does so for the specific industry and local market of small firm i . Specifically, it combines the outcome of the aggregate state s_{agg} with firm-specific conditions described by ε_i :

$$\pi_i = f(1 \cdot (s_{agg} = A) + \varepsilon_i)$$

and we restrict $f(\cdot)$ to be a monotonically increasing function with a range equal to the interval $[0,1]$.⁸ For simplicity, we assume that the project outcome for the large firm is directly dependent on s_{agg} as before, with the prior probability of an adverse aggregate state measured by π_0 . For the small firm, an adverse aggregate state increases the chances that the firm-specific economic state will also be adverse, but does not predict this perfectly.

⁸While we do not specify a functional form, the most widely-used options include the logistic function and the probit, or normal quantile function.

We maintain the same framework of observability as before, where prior distributions are common knowledge, but only the layoff decision is observed by the market.

The market updates the smaller firms' reputation in two steps. First, it updates its prior on the realization of s_{agg} , using the large firm's layoff decision. This, in turn, leads to an updated belief about π_i for the small firm, in turn impacting the reputational penalty the small firm would face if it announced layoffs of its own.

Using Bayes' Rule, the process of updating expectations about s_{agg} using the layoff decision of the large firm is straightforward. Letting π_0 be the prior expectation of the adverse state, the posterior expectation conditional on observing layoffs by the large firm, $\pi_1 \equiv \Pr[s_{agg} = A | \text{Large Firm Layoff} = 1]$ is given by:

$$\pi_1 = \pi_0 \left(\frac{1 - \eta(1 - \delta)}{1 - \eta(1 - \pi_0\delta)} \right) > \pi_0 \quad (2.13)$$

Note that the above updating rule is true for all possible strategies employed by the large firm, as long as we allow for trembles in the case where the large firm would like to always choose the *delay* policy. Moreover, while the market does not know the value of η for the large firm, taking expectations over any prior distribution leads to the same conclusion: $\pi_1 > \pi_0$. Thus, whenever the market observes layoffs by the large firm, its posterior beliefs imply that there is a higher chance that the aggregate state is adverse. This, in turn, increases the likelihood that the firm-specific economic state s_i will be adverse for the small firm. As a result, layoffs by the large firm lead to an increase in π_i as the market prepares to observe the action of the small firm.

Combining this result with the analysis in Figures 2.3 and 2.4, it follows that following a layoff by the large firm, the small firm will be more likely to choose the *terminate* policy. Intuitively, the layoff by the large firm means the market will be more willing to attribute poor performance to an adverse state rather than a lack of managerial talent, making further layoffs more likely. In effect, our model predicts a clustering or "safety in numbers" effect, where some firms will strategically announce layoffs close to the announcements of other firms, in groups in order to minimize the reputational costs they incur. In particular, we expect that firms whose characteristics normally push them toward the *delay* policy will be followers in such situations, announcing layoffs in the wake of firms whose characteristics push them toward the *terminate* policy. The following proposition summarizes this insight:

Proposition 2.2. *Firms tend to cluster layoff announcements after layoffs by a large firm.*

That is,

$$\frac{\Delta \Pr [Layoffs_i = 1 \mid \pi_i, \gamma, \sigma_\eta^2 C, \delta]}{\Delta (\text{Large Firm Layoff})} \geq 0.$$

Proof. See appendix. □

The mechanism described above can also occur in response to economic news that signals a deterioration of firm performance. Consequently, we expect that adverse aggregate news, correlated with real firm performance, will also trigger clustering of layoff announcements. This gives us the following corollary:

Corollary 2.3. *Firms cluster layoff announcements after negative macroeconomic news. The strength of the effect depends on the predictive power of the negative news with respect to firm performance. Specifically,*

$$\frac{\Delta \Pr [Layoffs_i = 1 \mid \pi_i, \gamma, \sigma_\eta^2 C, \delta]}{\Delta (\text{Adverse Macro Event})} \geq 0$$

and

$$\partial \left[\frac{\Delta \Pr [Layoffs_i = 1 \mid \pi_i, \gamma, \sigma_\eta^2 C, \delta]}{\Delta (\text{Adverse Macro Event})} \right] / \partial \left[\frac{\Delta p_i}{\Delta (\text{Adverse Macro Event})} \right] \geq 0$$

Proof. See appendix. □

Extending the analysis further, we turn to the types of firms which are more likely to respond to shocks such as layoffs by large firms and adverse macroeconomic shocks. Similar to the results of proposition 1 and 2, we find with strong reputation-based incentives and shorter track records are most likely to engage in layoff clustering. The following two corollaries summarizes these results:

Corollary 2.4. *If there is significant uncertainty about the manager's talent, then he is more likely to cluster layoff announcements after layoffs by a large firm:*

$$\frac{\partial}{\partial \sigma_\eta^2} \left[\frac{\Delta \Pr [Layoffs_i = 1 \mid \pi_i, \gamma, \sigma_\eta^2 C, \delta]}{\Delta (\text{Large Firm Layoff})} \right] \geq 0$$

Proof. See appendix. □

Corollary 2.5. *If the manager's utility function puts more weight on his reputation then he is more likely to cluster layoff announcements after leader layoff. Specifically:*

$$\frac{\partial}{\partial \gamma} \left[\frac{\Delta \Pr [\text{Layoffs}_i = 1 \mid \pi_i, \gamma, \sigma_\eta^2 C, \delta]}{\Delta (\text{Large Firm Layoff})} \right] \geq 0$$

Proof. See appendix. □

The propositions and corollaries in Section 3 summarize the testable predictions of the model. We now move to Section 4, which discusses how we link the parameters of the model to measurable attributes of firms and managers. With respect to the empirical tests, proposition 2 and its associated corollaries deal with leader-follower behavior, which we test using high-frequency data over short time horizons. By contrast, we test propositions 1 and its corollaries using lower-frequency data over the course of the business cycle.

2.3.6 Mapping the Model to the Data

The model presented in this section is a static three-period model. Therefore, in order to test the model's predictions we need to specify the appropriate time horizon. In principle, the time horizon depends on the persistence of beliefs about the economic state, and the corresponding persistence of reduced reputational costs to layoffs. Thus, to guide our empirical tests we choose the appropriate time horizon for our tests based on the frequency at which market's belief about the aggregate state of the economy changes. As summarized in the propositions above, the key comparative statics involve change in manager's behavior after the release of *adverse aggregate* news. Guided by this principle, our empirical tests are conducted at two frequencies: business cycle frequencies and daily frequencies. The timescale of business cycles is a natural candidate because market participants are much more pessimistic about the aggregate states during recessions relative to booms. Similarly, testing the predictions at daily frequencies is instructive, since release of unexpected bad news by a leading firm in an industry often drastically changes market's beliefs about the state of the industry in a matter of hours. The next section describes how we construct our

datasets, and then turns to the empirical strategies and results for both the business cycle frequency tests (Section 5), and the daily frequency tests (Section 6).

2.4 Dataset Construction

2.4.1 Layoff Announcements and Firm Characteristics

The data for this study are based on two sets of firms: large publicly-listed firms, and large privately-held firms. The publicly-listed firms are the population of firms in the annual Fortune 500 from 1970 to 2010. Analogously, the privately-held firms are the population of firms in the Forbes annual list of largest 100 privately held firms (“Forbes 100”) from 1985 to 2010. To minimize selection bias, we restrict the sample in any given year to the subset of firms that are contemporaneously constituents of the Fortune 500 or Forbes 100 in that year.⁹ Over the relevant range of years, we track 1013 different publicly-listed firms and 436 privately-held firms at an annual level. With this framework, we track announcements of layoffs by these firms in the Wall Street Journal, the definitive source of news for large US-based firms. For the publicly-held firms data from 1970 to 2006 comes from Kevin Hallock.¹⁰ Using the same methodology as Hallock (2009), we extended this dataset to 2010¹¹, and independently constructed a dataset of layoff announcements for the private firms from 1985 to 2010 (see data appendix for more details).

From the Wall Street Journal announcement dataset, we focus not only on the number of layoffs by a particular firm in a given year, but also track the total number of workers laid off. We then match our firms to four of COMPUSTAT’s datasets: Prices, Dividends, and Earnings; Fundamentals Annual, Fundamentals Quarterly, and ExecuComp. From the

⁹However, conducting our empirical analyses on the entire sample of firms that were *ever* in the Fortune 500 or Forbes 100 does not alter our key results.

¹⁰For related research using this data see Bilger and Hallock (2005) and Farber and Hallock (2009). Also, Hallock (2009) provides an interesting discussion of other aspects of this dataset which we do not explore.

¹¹There are several approaches to conducting searches on historical news database. In consultation with Kevin Hallock we narrowed the search criteria to three different methods. Using the three criteria we re-constructed the dataset for the publicly-held firms for three random years in the period 1970 and 2006. To ensure consistency we settled on the search criteria that yielded the maximum amount of overlap (i.e. greater than 98%) between the two datasets.

Prices, Earnings, and Dividends dataset, we obtain a firm's NAICS code, as well as information on its annual earnings and its equity: shares outstanding, market and book values, and dividends. From Fundamentals Annual, we obtain firm employment numbers and information from balance sheets and income statements: measures of debt, revenues, income, and capital expenditures. From Fundamentals Quarterly, we obtain date of earnings announcements, which serves as an important control variable in some of our empirical tests.

The data from ExecuComp is limited by the fact that it starts in 1992; however, it provides valuable information on the tenure and compensation of the CEOs of firms in our sample. We supplement this dataset with information from the Forbes CEO Compensation list of the largest 500 firms from 1970 to 1991. This allows us to construct measures of CEO tenure over several decades for a large subset of firms. This is critical for some of our empirical tests involving CEO tenure, as it allows us to include firm-fixed effects to examine within-firm variation.

In addition to these firm-specific measures, we also obtain sector-level data from the BLS Current Employment Statistics National Survey covering employment levels and number of hours worked, and measures of value-added from the National Income and Product Accounts of BEA, decomposed by NAICS major industry groups. We also obtain daily stock market returns from the CRSP database for the entire sample period, 1970 to 2010¹².

With these data, we first construct a range of standard control variables in order to cover a wide range of standard predictors of firm behavior. Specifically, the following variables are constructed based on firms' annual earnings reports covering the year prior to the layoff announcements being analyzed. We begin with the standard measures investors use to categorize companies into groups: firm size and value vs. growth. For the former, we include both the traditional market capitalization measure, as well as a measure of total firm value which combines equity market capitalization with the firm's long-term debt obligations. For the latter, we use both the ratio of equity book value to equity market value, as well as the earnings to price ratio for the firm's stock. In addition to these, we

¹²The data appendix goes into more detail about our methodology and procedures for constructing and merging the different datasets.

include a measure of financial leverage, equal to the ratio of the value of long-term debt obligations to the sum those obligations and the firm's equity. We also construct a measure of firm maturity as measured by years since initial public offering (IPO) date.

To test the propositions outlined in the theory section we construct two different datasets. In the first dataset each firm is tracked annually (Annual dataset), and in the second each firm is tracked every business day (Business Day dataset). Out of 5569 layoff announcements we only find two to be announced in the Weekend edition of the Wall Street Journal. Consequently, the Business Day Histories is at the business day level rather than the calendar day level¹³.

2.4.2 Confidential Microdata from the Mass Layoff Statistics Program of BLS

The Mass Layoff Statistics program (MLS) of the Bureau of Labor Statistics (BLS) is a Federal-State cooperative statistical effort which uses a standardized, automated approach to identify, describe, and track the effects of major job cutbacks, using data from each State's unemployment insurance database. Establishments which have at least 50 initial claims for unemployment insurance (UI) filed against them during a consecutive 5-week period are contacted by State agencies to determine whether the claimants are facing separations of at least 31 days duration, and, if so, information is obtained on the total number of separations, the reasons for these separations, and recall expectations. Establishments are identified according to industry classification and location, and unemployment insurance claimants are identified by demographic characteristics including age, race, sex, ethnic group, and place of residence. The data is collected at a monthly frequency starting in April of 1995. We end our sample in December 2010.

According the MLS definitions, a mass layoff occurs when at least 50 initial claims are filed against an establishment during a consecutive 5-week period. An extended mass layoff occurs when at least 50 initial claims are filed against an establishment during a consecutive 5-week period and at least 50 workers have been separated from jobs for more

¹³These two layoff announcements were recoded as occurring on the following Monday. Our results are identical when we drop these two observations.

than 30 days. Since extended mass layoffs are a better measure of layoffs that lead to more permanent job dislocation (greater than 30 days), we focus on this measure in our analysis.

Our focus on the subset of establishments employing 50 or more workers means that, according to the 2003 data, 4.6 percent of all covered employers and 56.7 percent of covered employment are in the program's scope.¹⁴ This measure has been quite stable over time: more than two decades ago, 5 percent of employers and 61 percent of total employment were reported in establishments with 50 or more workers (Brown 2004).

The Bureau of Labor Statistics keeps the identity of companies that engage in mass layoffs confidential. Under the auspices of the onsite researcher program of BLS, we were able to access the confidential microdata, which allowed us to extend our empirical analysis to actual mass layoffs, in addition to the layoff announcement observations based on the Wall Street Journal data. Five state employment offices, however, rejected our proposal to access the confidential data citing state legislation that disallows them to share the identity of establishments even for research purposes. Nevertheless, the researchers at the BLS estimate that the confidential data that was accessible to us covered more than 85% of all the mass layoff events they track. Having access to actual mass layoffs data allows us to examine the degree to which strategic behavior by firms can lead to actual changes in the labor market outcomes.

2.5 Business Cycle Frequency Tests

One of the main predictions of the model presented in Section 3 is that firms with managers who care most strongly about reputation are more likely to engage in layoffs during downturns (proposition 2 and 3). In an ideal experiment, we would estimate the magnitude of this effect using two identical firms, such that the manager of one firm has incentives to manage reputation while the other does not. In the absence of such an experiment, we exploit differences in the incentives faced by publicly-listed firms ("public" firms) and privately-held firms ("private" firms). Public firms differ from private firms along three

¹⁴The large difference in percentages reflects the strongly right-skewed distribution of employer size, where a relatively small fraction of establishments provide a majority of jobs.

major margins, all of which make their managers more likely to manage their reputation in financial markets, relative to a similar private firm. First, since public firms sell shares to outside investors who are not involved in managing the firm, there exists separation between ownership and control. This may lead to agency problems if managers' interests diverge from those of their investors (Jensen and Meckling (1976)). Second, owners of public firms typically have shorter horizons, since liquidity makes it easy for shareholders to sell their stock at the first sign of trouble rather than actively monitoring management. This relative myopic behavior of investors, weakens incentives for effective corporate governance (Bhide (1993)), and generates incentives for managers to be myopic in their reputation management (Stein (1989)). Third, managers of public firms are subject to takeover threats, which are, in part, dependent on the stock price of targeted firms. This can lead to managerial myopia in public firms in order to actively manage current stock prices (Stein (1988) and Edmans et. al (2009))¹⁵.

If reputation management drives layoff behavior over the business cycle, the first-order effect should show up when comparing differences in behavior of *similar* public and private firms. Here we emphasize reputation management in *financial markets*, since the managers of public and private firms are likely to have similar motivations for reputation management among other constituents. The next section describes the empirical strategy and data samples we use for our tests.

2.5.1 Comparing Public-Private Firms: Empirical Strategy

The analysis of this section is based on the confidential microdata collected at a monthly frequency from April 1995 to December 2010 by the Mass Layoff Statistics program of the Bureau of Labor Statistics. The dataset includes firms that ever engaged in a layoff during the sample period. Using this data we create three different samples for our study.

¹⁵Private firms, in contrast, are often owner-managed and even when not, are both illiquid and typically have highly concentrated ownership, which encourages their owners to monitor management more closely. Indeed, evidence from the Federal Reserve's 2003 Survey of Small Business Finances (SSBF) shows that 94.1% of the larger private firms in the survey have fewer than ten shareholders (most have fewer than three), and 83.2% are managed by the controlling shareholder. As a result, agency problems are likely to be greater among public firms than among private ones.

Full Sample

The construction of our full sample for this portion of our analysis begins with all public firms that are Fortune 500 constituents between 1985 and 2010; and all private firms that are Forbes 100 constituents between 1985 and 2010. We then match these firms to the microdata we accessed from the MLS database, resulting in a total of 478 public firms, and 135 private firms tracked over the period covered by MLS, namely, 1995-2010.¹⁶ We call this sample the “full sample.” Table 1 reports the characteristics of both the private and public firms in this sample. Over the 1995-2010 time period, the public firms tend to be larger than the private firms in terms of both revenue and number of employees. Also, the baseline layoff propensity of public firms is about 1.8 percentage points greater than the private firms, although we find no difference in the number of workers laid off by both these firms in a given mass layoff event. In an ideal world, we would like to compare the investment behavior of two otherwise identical firms that differ only in their listing status. To get closer to this ideal we need to find pairs of public and private firms that are observably similar to each other. One convenient way to do this is through matching, which is what we turn to next.

¹⁶The number of firms we track for this analysis is reduced by two factors: First, five states did not allow us to access their mass layoffs information. Second, not all firms in our broad sample engaged in layoffs between 1995 and 2010. This sample differs from our other results in that it considers non-contemporaneous constituents. The vast majority of our results are unchanged when restrict the sample to contemporaneous constituents of the two lists between 1995 to 2010, although this substantially reduces the sample size.

Table 2.1: Public-Private Firm Characteristics

TABLE 1: Public - Private Firm Characteristics

This table presents descriptive statistics for the full samples of public and private firms, for a size-and-industry matched sample, and for a sample of public and private firms that were successfully or unsuccessfully targeted by leveraged buyouts (LBO), over a period from 1995 and 2010. The table reports mean, median, and standard deviation of the key variables used in our empirical analysis that compares public-listed and privately-held firms. We also report pairwise differences in means, with *** and ** indicating a difference that is significant in a t-test at the 1% and 5% level, respectively.

	FULL SAMPLE			MATCHED SAMPLE			BUYOUT SAMPLE		
	Public Firms	Private Firms	Difference in Means	Public Firms	Private Firms	Difference in Means	Public Firms	Private Firms	Difference in Means
Revenue (in 2005 USD)									
Mean	15143.29	8067.53	7064.76***	7162.07	6992.29	169.78	9218.31	8609.35	608.97
Median	6035.37	4406.83		5075.34	4903.13		4871.72	5258.40	
St. Dev	30540.05	10817.58		5122.21	4951.14		14895.95	6995.246	
Employees									
Mean	46803.01	36624.15	10178.86***	29341.66	29614.29	-272.63	43101.70	46120.20	-3018.50
Median	21586	17649		19117	15000		21000	28000	
St. Dev	98809.90	57237.13		20212.44	26967.78		68204.04	44141.21	
Layoff Propensity									
Mean	0.0721	0.0541	0.0180***	0.0584	0.0558	0.0027	0.0689	0.0698	-0.0009
St. Dev	0.2587	0.2263		0.2345	0.2295		0.2533	0.2550	
Number Laid off									
Mean	337.74	357.88	-20.14	281.59	248.82	32.77	284.32	409.57	-125.25**
Median	174	200		160	150		166	191	
St. Dev	953.98	490.70		521.45	418.56		446.33	615.03	
Share Laid Off									
Mean	0.0134	0.0165	-0.0031	0.0138	0.0169	-0.0031	0.0134	0.0168	-0.0034
Median	0.0035	0.0060		0.0047	0.0056		0.0043	0.0056	
St. Dev	0.0819	0.0356		0.0132	0.0442		0.0686	0.0280	
No. of Firms	478	135		206	74		42	23	
No. of Observations	70665	10632		10470	10470		5502	1803	

Matching Sample

Since size is an important observable difference between the public and private firms in our sample, we match on size (revenue) in addition to matching on industry. This procedure closely follows the methodology of Asker et. al (2011), who conduct a similar matching between public and private firms to investigate differences in investment sensitivities. Matching on size implies that our matched sample consists of the bottom half of public firms in the Fortune 500, which correspond to the size of all private firms in the Forbes 100. (see Table 1 for a comparison).

In the language of the matching literature surveyed in Imbens and Wooldridge (2009), we use a nearest-neighbor match adapted to a panel setting. Starting in fiscal year 1985, for each public firm, we find the private firm that is closest in size and that operates in the same

three-digit NAICS industry, requiring that the ratio of their total revenue (TR) is less than 2 (i.e., $\max(TR_{public}, TR_{private}) / \min(TR_{public}, TR_{private}) < 2$). If no match is found, we discard that observation and look for a new match for that firm in the following year. Once a match is formed, it is kept in subsequent years to ensure the panel structure of the data remains intact. If a matching firm exits the panel, a new match is spliced in. Because we match with replacement, to maximize the match rate and match quality, the matched sample contains 206 public firms and 74 private firms. Our results are not sensitive to matching without replacement, although this substantially reduces the sample size. The standard errors are appropriately clustered to account for the resampling of private firms. The middle three columns in Table 1 compare the characteristics of the matched sample, and allows us to assess how good this match is. Since we match on size as measured by revenue, it is not surprising to find no statistical difference in the average revenue of public and private firms in our matched subsample. We find almost no difference in the average number of employees between public and private firms, and no difference in average layoff propensity or severity.

Leveraged Buyout Sample

Next, we create an alternate subsample based on leverage buyout (LBO) attempts. From the full sample we only keep private firms that were once public and went private through a LBO after 1985. We obtain this data from the Forbes annual survey of largest private firms in the US. As for the public firms, we track firms after 1985, and only include the public firms that were targeted by an unsuccessful LBO attempt.

In Table 1 we report the observed differences between public and private firms in this subsample. We again find no significant differences in revenue, number of employees, lay-off propensity, and the share of employees laid off between public firms (resulting from unsuccessful LBO attempts) and private firms (resulting from successful LBO attempts). We do find a difference in the average number of workers laid off, but the difference between medians is much smaller.

2.5.2 Comparing Public-Private Firms: Results

The main results of the analysis are reported in Table 2. We estimate the same set of two regressions using the three different samples described above. In each set the first regression has an indicator for mass layoff as the dependent variable, while the second regression uses the share of employees laid off (conditional on a layoff). All regressions include controls for the log of the previous year's revenues. Additionally, the regressions with layoff indicator as the dependent variable controls for the previous year's employee size. We are unable to control for other firm characteristics since the Forbes dataset only reports these two variables for private firms.

Full Sample Results

In the first set of specifications, (1) and (2), we estimate the regressions on the full sample, with no firm fixed effects or time fixed effects, but with 4-digit industry fixed effects. In addition we control for seasonality by including calendar month fixed effects, and for the overall time trend using a quadratic function. Using the BLS microdata for the period 1995 to 2010 at a monthly frequency, we find that private firms are roughly 2.01 percentage points more likely to layoff in a recession month. Compared to them, the public firms' propensity to lay off workers in recession months is 2.47 percentage points *greater*, indicating that the layoffs of public firms are more than twice as sensitive to recessions as those of private firms. The relatively modest response of private firms to recessions also shows up in the share of employees they lay off in recession years. Conditional on a layoff, private firms exhibit no difference in the share of employees they layoff in recession months and non-recession months. Contrastingly, the share of workers laid off by public firms goes up by 0.28 percentage points (conditional on a layoff) in a recession month when compared to the share laid off by private firms.

In the next set of specifications, (3) and (4), we conduct the same analysis but with firm fixed effects instead of industry fixed effects, and with month fixed effects instead of the controls for seasonality and time trends. We are still able to identify the impact of the private firm indicator, since this regression takes advantage of public-to-private and private-to-public transitions. During our sample years of 1995-2010, we have 38 such transitions.

The recession indicator in these regressions is not identified due to presence of month fixed effects. Under these specifications we find very similar results: a firm is more likely to announce layoffs in a recession if it is public. Similarly, when compared to private firms, public firms lay off more workers in recession years, although this coefficient is imprecisely estimated.

Table 2.2: Public-Private Comparison over the Business Cycle

Table 2: Public - Private Comparison over the Business Cycle

This table exploits within-firm or within-industry variation to analyze differences in actual layoff behavior over the business cycle between public and private firms. The unit of observation is a the firm level tracked monthly between April 1995 and December 2010. In this table, a layoff constitutes a mass layoff as defined by the Bureau of Labor Statistics under its Mass Layoff Statistics Program (see section 4.2 for further discussion). In this table we report four sets of regressions, each with two different dependent variables. The first dependent variable is layoff indicator, which takes a value of one if the firm engages in a mass layoff in a given month. The second dependent variable is the number of workers laid off as a share of previous year's employees. The first two set of regressions are estimated over the entire sample, which include firms that were in the Fortune 500 (public firms) or Forbes 100 (private firms) between 1985 and 2010. The only difference between the first and second set of regressions is that the first set includes 4-digit industry fixed effects instead of firm fixed effects, and a quadratic time trend and controls for seasonality (calendar-month fixed effects) instead of month fixed effects. On the other hand the second set includes firm fixed effects and month fixed effects, instead of industry fixed effects or controls for time trend. In the second set of regressions we are able to identify the effect of the private firm indicator variable since we have 38 cases of firms that transitioned from private-to-public or vice-versa. In the last two set of regressions, (5)-(8), we restrict the sample to matched public-private pairs (see section 5.1.2 for the methodology) based on size (revenue) and 3-digit NAICS industry. These specifications rely on comparing each public firm to its matched private counterpart since we include matched-pair fixed effects. All regressions include controls for previous year's log revenue and its interaction with the recession dummy, and previous year's number of employees and its interaction with the recession dummy (except when the dependent variable is share laid off). Since the month fixed effects absorb all variation over time, the effect of a recession indicator is not separately identified in the specifications that include month fixed effects. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Sample	Full Sample		Full Sample		Matched Sample		Matched Sample	
	Layoff Indicator (1)	Share Laid Off (2)	Layoff Indicator (3)	Share Laid Off (4)	Layoff Indicator (5)	Share Laid Off (6)	Layoff Indicator (7)	Share Laid Off (8)
Public Indicator	0.0051 (0.0085)	0.0020 (0.0013)	-0.0029 (0.0127)	0.0029 (0.0017)	-0.0064 (0.0090)	-0.0069*** (0.0025)	-0.0044 (0.0063)	-0.0060 (0.0037)
Recession Indicator	0.0201** (0.0087)	-0.0011 (0.0015)			0.0240** (0.0161)	0.0008 (0.0014)		
Public x Recession	0.0247*** (0.0097)	0.0028* (0.0015)	0.0259*** (0.0090)	0.0025 (0.0018)	0.0320** (0.0136)	0.0040* (0.0022)	0.0307** (0.0142)	0.0012 (0.0028)
Mean	0.0696	0.0137	0.0696	0.0137	0.0569	0.0155	0.0569	0.0155
Std. Dev	0.2544	0.0785	0.2544	0.0785	0.2318	0.0267	0.2318	0.0267
Industry Fixed Effect (4-digit NAICS)	✓	✓				✓		
Quadratic Time Trend & seasonality controls	✓	✓						
Month Fixed Effects			✓	✓			✓	✓
Firm Fixed Effects			✓	✓				
Matched-Pair Fixed Effects					✓	✓	✓	✓
Log Employees and Recession Interaction	✓		✓		✓		✓	
Log Revenue and Recession Interaction	✓	✓	✓	✓	✓	✓	✓	✓
Observations	81414	5587	81414	5587	20592	1167	20592	1167

Matched Sample Results

Our results using the matching estimator are reported in columns (5)-(8). These four specifications are a counterpart to the first four specifications discussed above. In addition to using our smaller matched sample, the key difference is in the control structure: we include a matched-pair fixed effect instead of the industry- and firm-level fixed effects in the previous specifications. Therefore, the identification in these regressions is based off

within-pair variation, where each pair consists of one public and one private firm within the same subindustry matched on size. The matching estimator results are in line with the results above: the layoff propensity of public firms is more than twice as sensitive to recessions as that of private firms, and the same is true for the share of employees laid off conditional on a layoff. Specifically, the propensity of private firms to engage in a mass layoff increases by 2.4 percentage points in recession months, while public firms experience an increase that is 3.2 percentage points *greater* than the effect we see in private firms. We also find that public firms are 0.64 percentage points less likely to lay off workers outside recession months when compared to private firms, although this result is not precisely estimated. We find similar results for the fraction of employees laid off conditional on a layoff. Outside recession months public firms lay off a relatively smaller fraction of employees compared to private firms, whereas the opposite holds in recession months. These coefficients, however, are not precisely estimated and cannot be interpreted as definitive results.

The general message from the matched sample results is that the public firms are more cyclical in their layoff policies compared to their matched private counterparts. These results suggest that public firms may be carrying excess capacity and waiting for longer periods between layoffs when compared to similar private firms. To investigate this further, we use the same matching methodology to estimate both the total number of workers laid off and the median duration between layoffs over the course of a full business cycle. We present these results in Table 3. In examining the total number of workers laid off, we use a peak-to-peak identification, starting from October of 2000 and ending in July of 2007. We report our findings in column (1): public firms tend to lay off approximately 30% more workers over the course of the entire business cycle, though this result is not precisely estimated. When we consider the median duration between layoffs, we use a slightly different time period. We begin our sample in April of 2002, approximately six months after the trough of the 2001 recession, and end in December of 2009. The motivation for this is to observe firm behavior in the period after they are most likely to have adjusted their labor force to their desired optimal level: most layoffs occur between the start and the trough of a recession, and the firms in our sample would have had ample opportunity to adjust their labor force. Using within-pair variation, we find evidence for a difference in layoff timing

between public and private firms, and report these results in column (2) of Table 3. We find that the average duration between layoffs for a sample of firms that engage in layoffs during the 2002-10 period is 7.76 months. Using within-pair variation, we find that the duration between layoffs for public firms is roughly 0.65 months greater than their matched private counterpart. While this effect is not precisely estimated, it is consistent with the view that public firms may wait longer to announce layoffs.

Table 2.3: Public-Private Comparisons over the Business Cycle - Matched Sample

Table 3: Public - Private Comparison over the Business Cycle (part II)

This table reports characteristics of the matched sample and examines differences in public and private firm behavior over the business cycle with respect to their layoff policies. The first specification (1) computes the total number of workers laid off by a firm over an entire business cycle (peak-to-peak from 6 months prior to the peak of the 2001 recession (October 2000) to 6 months prior to the peak of the 2008 recession (July 2007)). Correspondingly each firm has one observation in this sample. We control for matched pair fixed effects, and correspondingly the coefficient on the public indicator variable relies on a comparison of a public firm with its matched private counterpart. In specification (2) using the same data structure we study the median distance mass layoffs for each firm over an entire business cycle. For this specification we rely on a trough-to-trough period, since the period right after a recession is a more natural starting point for this analysis. Once again each firm has one observation in the sample. We again control for matched pair fixed effects so as to rely on within pair variation. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Sample	Matched Sample	
	workers laid off over business cycle (1)	Median Months Since Last Layoff (2)
Dependent Variable		
Public Indicator	0.3039 (0.4251)	0.0656* (0.0363)
Mean	2.5745	7.76
Std. Dev	3.1077	6.69
Matched-Pair Fixed Effects	✓	✓
Observations	236	260

Leveraged Buyout Targets Sample Results

The last set of results are estimated using a fixed effects specification with the firms in our LBO sample, and we report these results in Table 4. We find that after controlling for 4-digit industry category, log of revenues and log of employees, the layoff propensity of public firms increases by 6.04 percentage points in recession months when compared to private firms in the sample. Outside of recession months, however, we find that public firms are slightly less likely to engage in layoffs. Examining the share of workers laid off, we once again we find that public firms are likely to lay off a larger fraction of their workforce in a recession month when compared to a private firm, but the effect is not statistically significant. Overall, these results are consistent with those from our other samples: public firms are much more cyclical when compared to private firms.

Table 2.4: Public-Private Comparisons over the Business Cycle - LBO Targets

Table 4: Public - Private Comparison over the Business Cycle (part III)

This table analyzes differences in actual layoff behavior over the business cycle between public and private firms. The unit of observation is a the firm level tracked montly between April 1995 and December 2010. In this table, a layoff constitutes a mass layoff as defined by the Bureau of Labor Statistics under its Mass Layoff Statistics Program (see section 4.2 for further discussion). The set of regressions is estimated on a subsample of firms that were targets of a leveraged buyout (LBO). Among the targets the public firms are those for whom the LBO offer was withdrawn, and the private firms are those for whom the buyout offer was successful. The regressions include month fixed effects, and controls for previous year's log revenue and its interaction with the recession dummy, and previous year's number of employees and its interaction with the recession dummy (except when the dependent variable is share laid off). Since the month fixed effects absorb all variation over time, the effect of a recession indicator is not separately identified in these specifications. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Sample	LBO Targets	
	Layoff Indicator (1)	Share Laid Off (2)
Dependent Variable		
Public Indicator	-0.0079 (0.0261)	-0.0088 (0.0272)
Public x Recession	0.0604** (0.0258)	0.0181 (0.0176)
Mean	0.0692	0.0142
Std. Dev	0.2550	0.0610
Industry Fixed Effect (4-digit NAICS)	✓	✓
Month Fixed Effects	✓	✓
Log Employees and Recession Interaction	✓	
Log Revenue and Recession Interaction	✓	✓
Observations	7116	470

2.5.3 Alternate Explanations for the Difference in Public vs. Private Layoff Behavior

This section explores the plausibility of explanations other than reputation management for the difference in layoff propensity between public and private firms. The key con-

cern is that when we compare public and private firms, there are unobservable differences between unrelated to reputation management which may be driving the results presented above. Among the set of possible sources of unobservable heterogeneity between public and private firms, financial leverage and lifecycle effects are central and may directly alter the layoff behavior of firms independent of any reputation-management behavior of managers. Private firms tend to have greater degree of financial leverage compared to public firms, and also tend to be younger than public firms. Since we do not observe these characteristics for our sample of private firms we cannot control for them in our regressions. In order to assess the importance of these characteristics we instead compare our sample of private firms to the most levered public firms, and to the youngest public firms. If these characteristics are drivers of layoff policies, we would expect that in recessions, the layoff behavior of high-leverage public firms and young public firms will be quite similar to that of private firms. We investigate these alternate hypotheses in Table 5.

The dependent variable in all the regressions in Table 5 is the layoff indicator. Column 1 restricts the sample of public firms to ‘young’ firms (those whose time-since-IPO in their first year in our panel is less than the median time-since-IPO of all public firms in a given calendar year), while column 2 restricts the sample of public firms to ‘old’ firms. Similarly, Column 3 restricts the sample of public firms to ‘high leverage’ firms (those whose leverage ratio exceeds the median leverage ratio of all public firms in a given calendar year), while column 4 restricts the sample of public firms to ‘low leverage’ firms. In the last specification, we restrict the sample to public firms only, and estimate the interactions between the recession indicator and each of the two characteristics above: the log of years since IPO, and the leverage ratio. Each regression includes month fixed effects, the log of previous year’s employees and its interaction with recession indicator, and the log of the previous year’s revenue and its interaction with the recession indicator.

We find that the younger public firms are much more likely to lay off in a recession month compared to older public firms (specification (5)). We also find no significant effect of leverage on the sensitivity of layoff propensity to recessions. These results are consistent with specifications (1)-(4): the difference between public and private firms is strongest when comparing against young public firms, and is relatively consistent when comparing against high- and low-leverage public firms. The key implication of the results in this

table is that the observed differences in layoff propensity in recessions between public and private firms are not being driven by unobservable differences in either leverage or lifecycle effects.

Though these results rule out two key possible alternate explanations, there may be other forms of unobserved variation between public and private firms. In order to investigate this further, we refined our matching criteria to match on size and 4-digit subindustry (instead of 3-digit subindustry). This reduces our sample size by approximately 50% , but we have enough observations to conduct similar analysis as reported in Table 2. The results of this analysis is reported in Appendix Table 2. These results based on the four digit industry level replicate what we find in Table 2, and the magnitudes of the coefficients in this table line up with the analysis using a matching criteria based on the three digit industry level. Therefore, our results is robust to changes in the matching criteria we use. Moreover, using a more stringent matching criteria (i.e. at the four digit industry level) allows us to mitigate unobservable differences between public and private firms.

Table 2.5: Assessing Alternate Explanations

Table 5: Assessing Alternate Explanations for Public - Private Differences in Cyclicalty of Mass Layoffs

This table explores the plausibility of alternate explanations for the difference in layoff propensity between public and private firms. The dependent variable in all the regressions is the layoff indicator. Column 1 restricts the sample of public firms to 'young' firms (those whose time-since-IPO in their first year in our panel exceeds the median time-since-IPO of all public firms in the same calendar year), while column 2 restricts the sample of public firms to 'old' firms. Similarly, Column 3 restricts the sample of public firms to 'high leverage' firms (those whose leverage ratio exceeds the median leverage ratio of all public firms in the same calendar year), while column 4 restricts the sample of public firms to 'low leverage' firms. In the last specification, we restrict the sample to public firms only, and estimate the interaction between the recession indicator and, both, log of years since IPO and leverage ratio. Each regression includes month fixed effects, log of previous year's employees and its interaction with recession indicator, and log of the previous year's revenue and its interaction with the recession indicator. The first four specifications include industry fixed effects, whereas the last one includes firm fixed effects. Since the month fixed effects absorb all variation over time, the effect of a recession indicator is not separately identified in these specifications. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Sample	Young Firms	Old Firms	High Leverage Firms	Low Leverage Firms	Public Only
Dependent Variable	Layoff Indicator (1)	Layoff Indicator (2)	Layoff Indicator (3)	Layoff Indicator (4)	Layoff Indicator (5)
Public Indicator	0.0095 (0.0154)	0.0055 (0.0231)	0.0009 (0.0139)	0.0071 (0.0196)	
Public x Recession	0.0478*** (0.0127)	0.0133 (0.0099)	0.0187* (0.0108)	0.0250** (0.0108)	
Leverage Ratio					0.0721*** (0.0137)
Log(1+Years since IPO)					0.0005 (0.0136)
Recession X Lev. Ratio					0.0259 (0.0190)
Recession X Log Age					-0.0341*** (0.0116)
Mean	0.0567	0.0785	0.0602	0.0759	0.0718
Std. Dev	0.2314	0.2689	0.2379	0.2639	0.2582
Firm Fixed Effects					✓
Industry Fixed Effect (4-digit NAICS)	✓	✓	✓	✓	
Month Fixed Effect	✓	✓	✓	✓	✓
Log Emp. and Interaction with Rec. Indicator	✓	✓	✓	✓	✓
Log Rev. and Interaction with Rec. Indicator	✓	✓	✓	✓	✓
Observations	44805	47019	45078	46746	69621

2.5.4 Variation within Public Firms

So far, our analysis has been based on comparing public and private firms. In this section we look for differences in layoff behavior within our sample of public firms. In seeking to identify managers that are more likely to engage in reputation management and time

layoffs strategically, we rely on the following two measures. First, we use an indicator variable called short-tenured CEO, which takes a value of 1 if the CEO's tenure with the firm is four years or less. Second, we measure a firm's past equity-linked compensation share as the share of total CEO compensation that comes from equity-linked instruments over the past 5 years. We are interested in the impact of these variables on actual layoff behavior over the business cycle, and we report the results in Table 6. We use the same set of firm-level controls as in Table 5, and also include month fixed effects and firm fixed effects. The sample includes all contemporaneous constituents of the Fortune 500 that ever engaged in a layoff. In specification (1) we find that short-tenured CEOs are roughly 1.44 percentage points more likely to engage in a layoff in a recession month, relative to firms with longer-tenured CEOs. Similarly, in specification (2), we find that a one-standard-deviation increase in the share of equity-linked compensation (controlling for the level of total compensation) is associated with an increase of roughly 0.7 percentage points in layoff propensity during a recession month.

In specification (3) we restrict the sample to all firms that have option share above the median level for a given year. Similarly, in specification (4) we restrict the sample to all firms that have CEO tenure below the median level for a given year. We estimate the same regressions as in specifications (1) and (2), and find that our results get stronger: both short-tenured CEOs and equity-linked compensation are linked to a greater incidence of layoffs during recessions. These results suggest that within the set of public firms, the CEOs who are most likely to care strongly about reputation are also the ones who are most likely to engage in layoffs during recessions. This suggests that reputation management by CEOs plays a significant role in determining the cyclicity of their firms' layoff policies.

Table 2.6: Actual Layoff Propensity over the Business Cycle

Table 6: Actual Layoff Propensity over the Business Cycle (Public Firms), 1995-2010

This table exploits within industry variation to analyze the impact of the equity-linked executive compensation and short tenure of CEO on layoff propensity over the business cycle. The unit of observation is at the firm level tracked monthly between April 1995 and December 2010. In this table, a layoff constitutes a mass layoff as defined by the Bureau of Labor Statistics under its Mass Layoff Statistics Program (see section 4.2 for further discussion). The sample for the first two specifications includes all contemporaneous constituents of Fortune 500 that ever announced a layoff. In specification (3) we restrict the sample to all firms that are above the median with respect to option share of compensation in the previous year. Similarly, in specification (4) we restrict the sample to all firms above the median with respect to CEO tenure in the previous year. The dependent variable is an indicator variable, which takes a value of one when a firm announces a layoff in a given month, and zero otherwise. In each regression specification the main explanatory variables are the recession indicator, key variable, and the interaction of the key variable with the recession indicator. The recession indicator takes a value of one in months classified as recession months by the NBER Business Cycle Dating Committee. The first key variable (specification (1) and (3)) is a measure of short-tenured CEO, which is an indicator variable that takes a value of one if the tenure of a given CEO at the firm is between 0 and 4 years. The second key variable (specification (2) and (4)) is a measure of equity-linked compensation, which is the Black-Scholes past five year average of the value of stock-option grants a CEO receives as a share of the average total compensation. All the specifications include month fixed effects, industry fixed effects, and firm-level controls and the interaction of each of these controls with the recession indicator. The firm level controls include Earnings-Price ratio, Book-to-Market Ratio, Market Capitalization, Leverage Ratio, Years since IPO, which are all obtained from COMPUSTAT. Since the month fixed effects absorb all variation over time, the effect of a recession indicator is not separately identified in these specifications. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Sample	Full Sample		High Option Share Firms	Short-tenured CEO Firms
	Layoff Indicator (1)	Layoff Indicator (2)	Layoff Indicator (3)	Layoff Indicator (4)
Dependent Variable				
KV = Short CEO Tenure Indicator	0.0032 (0.0050)		0.0025 (0.0062)	
KV = Avg. Equity-Linked Compensation Share		0.0072 (0.0071)		0.0164 (0.0091)
Key Variable (KV) x Recession	0.0144* (0.0079)	0.0261** (0.0126)	0.0255** (0.0123)	0.0352** (0.0168)
Mean	0.0787	0.0790	0.0812	0.0831
Std. Dev	0.2693	0.2697	0.2732	0.2761
Industry Fixed Effects	✓	✓	✓	✓
Month Fixed Effects	✓	✓	✓	✓
Firm Controls and Recession Interaction	✓	✓	✓	✓
Observations	28746	27882	16818	13410

2.6 Daily Frequency Tests

In this section we turn to the daily frequency tests of our model's predictions. First we establish that the reputation penalty is lower if a firm announces a layoff right after other large firms in the economy announce layoffs (Section 6.1). We evaluate the strength of the response to this reduction in reputation penalty when we test Proposition 4, which predicts that firms will cluster layoffs after layoff announcements by large firms. Finally, we test the differential sensitivity results in Corollaries 6 and 7, which predict that strategically-motivated managers (those with high γ and σ_η^2) will be more likely to cluster their layoff announcements.

2.6.1 Is the Reputation Penalty of Layoff Announcements Lower after Layoffs by Other Firms?

Though the manager may be managing his reputation with several constituents—the stock market, the board of directors, employees of the firm—the analysis in this section focuses on financial market reputation, which we can test this using daily stock returns. This focus on stock-market-based measures is driven by ease of testability, and should not be construed as a narrow interpretation of the reputation management mechanism in our model. Several studies have documented a negative stock market reaction to layoff announcements (Farber and Hallock (2001), Hallock (2009)). In this section we are interested in whether this negative penalty is lower if a firm announces a layoff within days of a layoff announcement by a large firm. Our empirical strategy follows the conventional event-study approach.

Using a sample of the Fortune 500 constituents from 1970 to 2010, we calculate cumulative excess returns using value-weighted return data from the Center for Research in Security Prices (CRSP) at the University of Chicago. The excess return is the part of the movement in the stock return of a company that is not correlated with overall market movement in stock returns and presumably reflects unexpected firm-specific factors. To do this we run a first-stage regression where the daily stock return for company i on day t , denoted

by R_{it} , is regressed on the value-weighted return of the market, R_{mt} :

$$R_{it} = \alpha_i + \beta_i R_{mt} + \eta_{it}$$

Next, for days around the event, the daily abnormal (or excess) returns is calculated as follows:

$$ER_{it} = R_{it} - \left(\hat{\alpha}_i + \hat{\beta}_i R_{mt} \right)$$

where $\hat{\alpha}_i$ and $\hat{\beta}_i$ are estimates of the previous regression. The first stage regression is run for a period in the past, which in this study ranged for a period of one year ending 30 days before the event. We rely on the average cumulative excess return over a five-day window—two days before, the day of, and two days after the event for each of the 41 years from 1970 to 2010. Changes to this window length has no material effect on the results. The results are listed in Table 7. Specification (1) restricts the sample to all layoff announcements that occur within the three days following a layoff by the largest 20 public firms in the economy as measured by the previous year's revenue. Specification (2) considers the complementary case, in which the sample includes layoff announcements that occur on all other days.

Table 2.7: Stock Penalty of Layoffs

Table 7: Stock Penalty of Layoffs: Cumulative Abnormal Returns (from day -2 to day +2)

This table reports the cumulative abnormal returns (CAR) around a layoff announcement. The sample includes daily observations from 1970 to 2010. The baseline CAR is reported in specification (1) and (3) against the constant term of the regression. The main coefficient of interest is associated with the indicator variable reported in the first line, which takes a value of one if the firm under observation announces a layoff within three days after a layoff announcement by the largest 20 firms (as measured by previous year's revenue). In specifications (3) and (4) we also include an indicator variable for whether there was a layoff announcement by the same set of largest 20 firms within the three days following the layoff announcement by the firm under observation. Specification (2) and (4) also includes year fixed effects and industry fixed effects. Inclusion of these fixed effects implies that we cannot estimate the constant term in these specifications. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dependent Variable	Cumulative Abnormal Return	Cumulative Abnormal Return	Cumulative Abnormal Return	Cumulative Abnormal Return
	(1)	(2)	(3)	(4)
Layoff by Top 20 Firm in Previous 3 days = 1	0.0049*** (.0017)	0.0052*** (.0019)	0.0042** (.0020)	0.0044** (.0021)
Layoff by Top 20 Firm in Next 3 days = 1			-0.0025 (.0029)	-0.0027 (.0029)
Constant (Baseline CAR)	-0.0087*** (0.0013)		-0.0081*** (0.0016)	
Year Fixed Effect		✓		✓
Industry Fixed Effect		✓		✓
Observations	4796	4796	4796	4796

We find that the cumulative excess return around a layoff announcement that occurs within the three days following a top 20 firm announcement is -0.38 percentage points. On the other hand, if a layoff occurs on any other day, the cumulative excess return is more than twice as large: -0.87 percentage points. This suggests that in the context of financial markets, the reputation penalty of layoffs is lower immediately after negative signals about the state of the economy. In the next section, we proceed to test whether firms respond to these incentives by timing their layoff announcements to occur immediately after a layoff announcement by top-20 firms.

2.6.2 Do Firms Announce Layoffs after other Large Firm Layoff Announcements?

Empirical Strategy

To assess whether firms engage in a ‘leader-follower’ behavior with respect to layoff announcements, our estimation strategy relies on a dynamic regression model with lagged and future effects. For a firm i at time t , the regression specification is

$$Layoff_{it} = \alpha_i + \sum_{j=-p}^p \beta_j MacroEvent_{g,t-j} + X'_{it}\phi + Y'_t\omega + \varepsilon_{it} \quad (2.14)$$

The central variables are $Layoff_{it}$, an indicator variable which takes a value of one when firm i announces layoff on business day t , and $MacroEvent_{g,t-j}$, which is an indicator variable that takes a value of one if there was a negative macroeconomic news released on date $t - j$ relevant for a firm in industry g . We also include firm-level controls, denoted by X'_{it} to control for firm-level heterogeneity. We begin our analysis by focusing on the layoff announcements of public firms. For public firms, these controls include total revenue, number of employees, years since IPO, book-to-market ratio, earnings-price ratio, leverage ratio, and days since last earnings announcement.¹⁷ For private firms, we include the two control variables that we are able to observe: total revenues and number of employees. In addition to concerns about firm heterogeneity, we also need to account for the possibility that firms may be more likely to announce layoffs on certain days of the week (e.g. Friday) or in certain months. To address these concerns, the vector Y_t includes year fixed effects, month fixed effects, and day-of-week fixed effects.

We consider three different types of macro events: layoff by a top 20 firm (as measured by previous year’s revenue), layoff by a firm in the top 20 which shares the same 1-digit NAICS code as firm i , and, as a placebo test, unexpected negative news announcements that are not directly linked to economic performance. We conduct this analysis separately for public firms and private firms. In the first measure, which we label “Leader Layoffs,”

¹⁷Controlling for earnings announcement date is important since firms may be clustering layoff announcements around earnings announcement date, and we may observe clustering merely because different firms have earnings announcement dates close to each other.

we restrict our group of large firms to the largest 20 firms as measured by previous year's revenue.¹⁸ This ensures that the layoff announcements of these firms correspond closely with the notion of 'negative macroeconomic news' in our model. This measure takes the form of an indicator variable equal to one on the business day the WSJ reports any such layoff announcement. Our second measure, which we label "Industry Leader Layoffs," is similar in structure. It takes a value of one whenever the first measure takes a value of one *and* the large firm is in the same 1-digit NAICS industry as firm i . The final measure, which we label "News Media Events", is based on the 'biggest news stories' as measured by press coverage. Using a survey of major news events from USA today (2007), we construct a list of events for the period 1970 to 2010. Events are selected to be negative in nature (e.g. the Sept. 11 attacks), and to occur over a span of a day or less (i.e. news events such as the Afghanistan invasion of 2001 are excluded). As above, this measure is an indicator variable that takes the value of one on days when these events occur.

Our model predicts a strong asymmetry in layoff behavior before and after a large-firm layoff announcement. This is in sharp contrast to the common shock hypothesis, which predicts layoff announcements by smaller firms both before and after the leader layoff. The event study framework also enables us to tackle the issue of reverse causality: a potential concern is that smaller firms may drive large firm layoff announcements. Therefore, the coefficient on future events will enable us to establish whether this mechanism is at play. We report results for a lag length of $p = 5$, but note that our results are almost identical when we choose $p = 10$ or $p = 15$. To ensure comparability across all the regressions the sample of public firms is restricted to all contemporaneous constituents of the annual Fortune 500 list excluding the top 20 firms. Similarly, our sample of private firms include all contemporaneous constituents of the annual Forbes 100 list.

¹⁸Press and analyst coverage of publicly-listed firms is highly skewed, with the largest firms receiving an inordinately high degree of coverage compared to slightly less large firms (Fang and Peress (2007)). Our results are almost identical when we use a threshold of 5, 10, 25 or 30 for classification of large firms instead of the 20 largest firms measured by previous year's revenue.

Results: High Frequency Announcement Behavior of Firms

Figure 2.5 plots the sequence of β_j estimates from the event study specifications (2.14), along with point-wise 95% confidence intervals using standard errors clustered at the firm level. Panels A and B report the response to all “Leader Layoffs” by public and private firms, respectively. In Panel A, we see that the sequence of β_j estimates is roughly flat and close to zero before the leader layoff announcement ($j < 0$), and then jumps discretely at $t = 0$, and thereafter decreases gradually over the next 5 business days. Thus, a large firm layoff announcement is associated with future layoffs by other large public (Fortune 500) firms, but not with past layoffs. The magnitudes of these β_j should be compared to the unconditional average daily layoff announcement propensity of 0.0008. In Panel B, by contrast, we find no similar response to “Leader Layoffs” by private firms. Specifically, we find no evidence of clustering either before or after a top 20 layoff announcement. This is consistent with our business cycle frequency results, in which the public firms exhibited much greater propensity to engage in actual layoffs in recession months compared to the private firms.

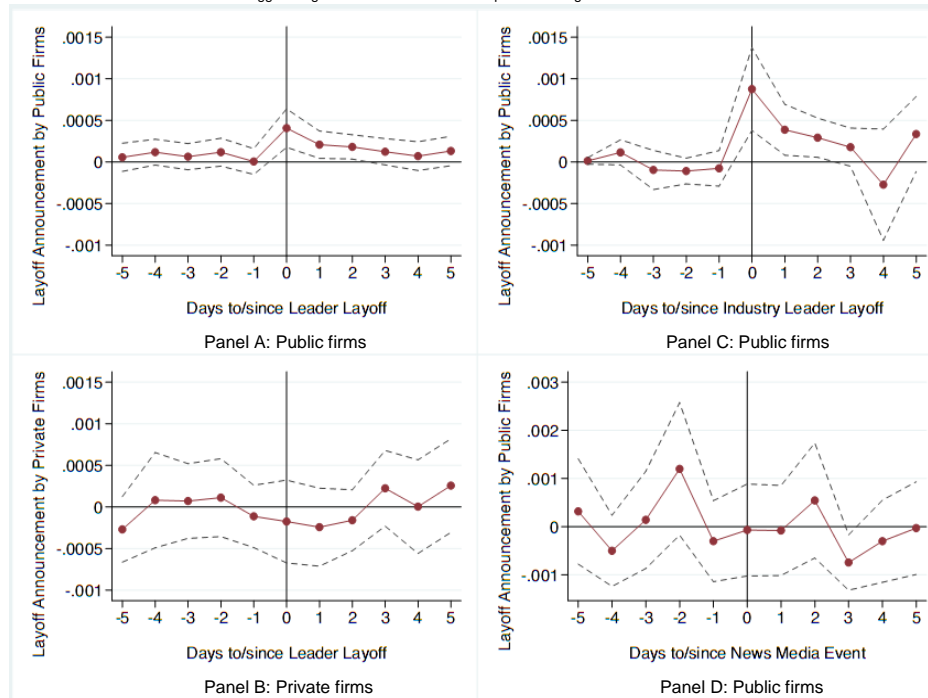
In Panel C, we return to public firms and investigate the response to “Industry Leader Layoffs.” We again find the pattern of no effect prior to the event date, followed by a strong jump and gradual decline at $t = 0$. Notably, the magnitude of the within-industry response of public firms to a layoff by a large firm is almost twice that of the economy-wide response described in Panel A. In Panel D, we turn our focus to the response of public firms to “News Media Events.” In contrast to events based on large-firm layoffs, a large negative media event does not predict future or past layoff announcements by the Fortune 500 firms. This suggests that firms are not attempting to time layoff announcements during periods when investors may be distracted by non-economic events.

Taken together, these results suggest that the Fortune 500 firms are more likely to time their layoff announcements in the days immediately after negative economic news is released, such as the aftermath of a layoff announcement by a very large firm. The strength of the ‘follower’ behavior is stronger when the negative news is more related to the firm’s own productivity; and the effect is absent after large negative news that are non-economic in nature. Therefore, these results are more in line with the leader-follower mechanism be-

ing driven by an informational channel, rather than a ‘hiding behind the headlines’ channel. Moreover, the asymmetric response of the firms before and after the large layoff announcements offer evidence against the ‘common shock’ hypothesis, which would predict a more symmetric response in layoff announcement responses of the Fortune 500 firms.

Figure 2.5: Event Studies of Leader Layoffs and News Media Events

Figure 2.5: These graphs report the response of layoff propensities to “Leader Layoffs” - layoff announcements by the largest 20 firms in the economy, based on previous year’s revenue. We plot the coefficients of dynamic regressions which predict daily layoff propensities over the 11-day event-time periods surrounding large-firm layoff announcements. In all cases, we include firm-level controls, and fixed effects at the annual, monthly, and day-of-week levels. We also include point-wise 95% confidence intervals, based on standard errors clustered at the firm level. In Panel A, we examine the response of layoff propensity for the public Fortune-500 firms in our dataset. We juxtapose this with the results in Panel B, where we plot the layoff propensity response for Forbes-100 firms. In Panel C, we replicate the results of Panel A but restrict our coefficient analysis to firms which are in the same 1-digit NAICS industry as the leader firm for each “Leader Layoff” event. In Panel D, we plot the response of all public Fortune-500 firms to “News Media Events” - the biggest negative news stories based on press coverage from 1970 to 2010.



2.6.3 Which Type of Firms Layoff after a Large Firm Layoff Announcement?

Empirical Strategy

To identify the characteristics of firms that announce layoffs immediately after a large firm layoff announcement, we construct an annual dataset in which the unit of observation is at the firm level for every year. With this framework, we aim to test the model’s pre-

dictions on the type of firms which are most likely to behave strategically. Specifically, corollaries 6 and 7 predict that managers for whom the market lacks strong priors, and who care most about reputation, will be most likely to engage in strategic layoff timing. These results rely on public firms since we do not observe clustering behavior for private firms. The basic regression specification is:

$$\begin{aligned} y_{it} &= \alpha_i + Z'_{it}\phi + V'_t\omega + \varepsilon_{it} \\ z_{it} &= \alpha_i + Z'_{it}\phi + V'_t\omega + \varepsilon_{it} \end{aligned} \quad (2.15)$$

In these regressions, y_{it} is an indicator variable which takes a value of one when a firm is a ‘follower’ firm and zero otherwise; analogously, z_{it} is an indicator variable which takes a value of one when a firm is a ‘counterfactual follower’ firm and zero otherwise. A follower firm is defined as any firm that announces a layoff in a 5-day window following a large layoff (including the day of the large layoff). Similarly, a counterfactual follower firm is a firm that announces a layoff in a 5-day window prior to a large layoff (excluding the day of the large layoff). The first set of regressions (specification (1) and (2)) is based on all firms in the dataset. This effectively treats all other Fortune 500 constituents as the control group. The second pair of regressions (specification (3) and (4)) restricts the sample to all firms that announce a layoff in year t . In effect, the control group in these specifications is the set of firms which also announced layoffs, but were outside the ten-day window which identifies firms as followers or counterfactual followers. In the last column (specification (5)), we restrict the sample to all firms that announce a layoff within 5 days (before or after) a large firm layoff announcement. In this last specification, the control group is simply the counterfactual followers: firms that announced layoffs in the five days *prior* to a large-firm layoff.

In all five specifications, the vectors Z_{it} and V_t include the same control variables and explanatory variables used in Table 6 and Figure 2.5: total revenue, number of employees, years since IPO, book-to-market ratio, earnings-price ratio, leverage ratio, and days since last earnings announcement. We also include a full set of time-based (annual) fixed effects.

In a closely-related test, we examine whether clustering at these short horizons is driven by information. Specifically, we test whether firms change their clustering behavior after they start being covered by financial analysts. We estimate the same set of five speci-

cations as described above, and include an indicator variable for analyst coverage as an explanatory variable. Using I/B/E/S sell-side analyst recommendations for U.S. stocks from 1993 to 2010, we construct an aggregate analyst coverage indicator variable. I/B/E/S codes recommendations from 1 (strong buy) to 5 (sell). We first restrict our sample to all firms that appear at least once in the I/B/E/S database. Next, we create an indicator variable, ‘Past 3 years coverage,’ which takes a value of 1 if an analyst covered by the I/B/E/S dataset made a recommendation in the previous three years. We report the results on determinants of follower behavior in Tables 8 and 9, with the former focusing on compensation structure and CEO tenure, while the latter focuses on analyst coverage.

Results

The key independent variables in Table 8 are the same ones we used in the business cycle frequency results of public firms in Table 6: an indicator variable for short-tenured CEO and the average of the share of CEO compensation that derives from equity-linked compensation over the past 5 years.

The results in this panel suggest that firms with a higher degree of equity-linked compensation are more likely to be followers; by contrast, this variable has no predictive power for counterfactual followers. Similarly, firms with short-tenured CEOs are more likely to be followers, while the same quality has a weak negative effect on the likelihood of being a counterfactual follower. These results are robust to the estimation using the full sample (specifications (1) and (2)), or the more restricted samples in specifications (3)-(5), although the coefficients are less precisely estimated due to reduced sample size. Overall, we find that the same characteristics which predict layoff cyclicalities on a business-cycle level also predict strategic behavior over shorter horizons: firms with short-tenured CEOs and with significant equity-linked compensation are much more likely to act as follower firms, but not as counterfactual followers. This suggests an important role of reputation management in driving the high degree of observed ‘leader-follower’ behavior in layoff announcements.

Table 2.8: Follower Characteristics

Table 8: Follower Characteristics, 1970-2010

In this table we report our results about the characteristics of public firms that layoff before and after the largest 20 firms in the economy as measured by previous year's revenue. Correspondingly the sample is restricted to the Fortune 500 constituents that are not the largest 20 firms as measured by revenue. The unit of observation is at the firm level observed over every business day between 1970 and 2010. There are two indicator variables that serve as dependent variables in the analysis. The follower indicator takes a value of one if in a given year the firm announces the layoff in a 5-day window following a large firm layoff (including the day of the large layoff). Similarly, the counterfactual follower indicator takes a value of one if in a given year the firm announces a layoff in a 5-day window prior to a large layoff (excluding the day of the large layoff). The first pair of regressions ((1) and (2)) are estimated using the entire sample (i.e. all Fortune 500 firms not in the top 20). The second pair of regressions ((3) and (4)) are estimated using only the sample of firms that laid off in a given year. By restricting the sample to this pool of firms, we are effectively relying on variation within firms that have relatively high propensity to layoff. In the last specification (5), we restrict the sample even further and include only firms that laid off within 5 days before or after a large firm announcement. Since in this case the two dependent variables are perfectly negatively correlated, we just need to estimate one regressions (instead of the pair). All the specifications include year fixed effects and firm level controls. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dependent Variable	Follower Indicator (1)	Counterfactual Follower Indicator (2)	Follower Indicator (3)	Counterfactual Follower Indicator (4)	Follower Indicator (5)
Sample	All Firms	All Firms	All Firms that Laid Off in Given Year	All Firms that Laid Off in Given Year	All Firms that Laid off within 5 days before/after large firm layoff
Avg. Equity-linked Compensation Share	0.0741*** (0.0186)	0.0186 (0.0165)	0.1850 (0.1546)	-0.1086 (0.1415)	0.4689* (0.2833)
Total Compensation	-0.0014*** (0.0004)	0.0004 (0.0011)	-0.0043 (0.0029)	0.0110*** (0.0025)	-0.0073* (0.0038)
Ceo Tenure = 0 - 4 years	0.0229*** (0.0075)	-0.0042 (0.0056)	0.1458** (0.0583)	-0.0945* (0.0496)	0.2534*** (0.0928)
Mean	0.0292	0.01763	0.2764	0.1645	0.6238
Std. Dev	0.1685	0.1316	0.4482	0.3715	0.4869
Year Fixed Effects	✓	✓	✓	✓	✓
Firm Level Controls	✓	✓	✓	✓	✓
Observations	2151	2151	228	228	101

To further explore the role of asymmetric information and reputation management, Table 9 reports our results from the analyst coverage regressions. The specifications here are identical to those in Table 8, except here the key explanatory variable is the indicator variable of analyst coverage. The results are broadly similar: when a firm is being covered by analysts, it is much less likely to announce layoffs after a large firm layoff. At the same time, the analyst indicator has no predictive power for counterfactual follower firms.

We interpret these results as supporting the idea of asymmetric-information based factors driving the leader-follower behavior.

Table 2.9: Follower Characteristics - Analyst Coverage

Table 9: Follower Characteristics, 1970-2010 (part II)

In this table we conduct the same analysis as in Table 8, except now our key dependent variable is a measure of analyst coverage. The sample is still restricted to the Fortune 500 constituents that are not the largest 20 firms as measured by revenue. The unit of observation is at the firm level observed over every business day between 1970 and 2010. There are two indicator variables that serve as dependent variables in the analysis. The follower indicator takes a value of one if in a given year the firm announces the layoff in a 5-day window following a large firm layoff (including the day of the large layoff). Similarly, the counterfactual follower indicator takes a value of one if in a given year the firm announces a layoff in a 5-day window prior to a large layoff (excluding the day of the large layoff). The first pair of regressions ((1) and (2)) are estimated using the entire sample (i.e. all Fortune 500 firms not in the top 20). The second pair of regressions ((3) and (4)) are estimated using only the sample of firms that laid off in a given year. By restricting the sample to this pool of firms, we are effectively relying on variation within firms that have relatively high propensity to layoff. In the last specification (5), we restrict the sample even further and include only firms that laid off within 5 days before or after a large firm announcement. Since in this case the two dependent variables are perfectly negatively correlated, we just need to estimate one regressions (instead of the pair). All the specifications include year fixed effects and firm level controls. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dependent Variable	Follower Indicator (1)	Couterfactual Follower Indicator (2)	Follower Indicator (3)	Couterfactual Follower Indicator (4)	Follower Indicator (5)
Sample	All Firms	All Firms	All Firms that Laid Off in Given Year	All Firms that Laid Off in Given Year	All Firms that Laid off within 5 days before/after large firm layoff
Analyst Coverage in Past 3 years	-0.0254*** (0.0093)	-0.0068 (0.0060)	-0.2061** (0.0898)	-0.0476 (0.0777)	-0.1222 (0.1935)
Mean	0.0361	0.0246	0.2464	0.1836	0.5940
Std. Dev	0.1864	0.1550	0.4439	0.3874	0.4916
Firm Fixed Effects	✓	✓	✓	✓	
Year Fixed Effects	✓	✓	✓	✓	✓
Firm Level Controls	✓	✓	✓	✓	✓
Observations	7185	7185	964	964	436

2.7 Linking High Frequency Results to Business Cycle Frequency Results

The high frequency results in the previous section offer strong evidence for strategic behavior, but on their own, they do not indicate the existence of significant welfare-relevant effects. In order to assess this, we investigate the impact of being a *follower* firm on changes in layoff propensity over the business cycle. Being a high-frequency follower in layoff announcements is, in principle, a better measure of active reputation management than other measures we have used (such as short-tenured CEO and equity-linked compensation). This is because we can rely on direct observations of reputation management behavior rather than predictions of such behavior. The results in this section seek to establish a connection between our high frequency results and the business-cycle-frequency results described in Section 5.

For this analysis, we first identify firms that have been ‘follower’ firms in the past five years, using the same methodology as in the previous section. This is our basis for our measure ‘Past 5 year follower,’ which takes the value of one for a specific firm in a given calendar month if, at any point over the prior five years, that firm has engaged in a layoff announcement within the five days following a layoff announcement by a large (i.e. top-20) firm. Analogously, we create a measure of ‘Past 5 year counterfactual followers,’ which takes a value of one for a specific firm in a given calendar month if, at any point over the prior five years, that firm has engaged in a layoff announcement within the five days prior to (and not after) a layoff announcement by a large firm.

The results of this analysis are reported in Table 10. We find that outside of recession months the propensity of follower and counterfactual follower firms are statistically indistinguishable. However, within recession months, a firm that has been a follower firm in the previous five years is roughly 3.14 percentage points more likely to engage in a mass layoff. By contrast, we find the impact of recessions on layoff propensity is not statistically different from zero for counterfactual follower firms. These results suggest that the firms we identify as ‘strategic’ over the very short horizons of our daily frequency data are also the firms that are more likely to have cyclical layoff policies over the course of the business

cycle.

Table 2.10: Linking Daily and Business Cycle Outcomes

Table 10: Linking Daily Frequency Layoff Behavior to Business Cycle Outcomes, 1995-2010

This table examines whether firms that engage in high frequency clustering of layoff announcements also are the ones that are more likely to layoff in recessions. The unit of observation is at the firm level tracked monthly between April 1995 and December 2010. In this table, a layoff constitutes a mass layoff as defined by the Bureau of Labor Statistics under its Mass Layoff Statistics Program (see section 4.2 for further discussion). The dependent variable is an indicator variable, which takes a value of one when a firm announces a layoff in a given month, and zero otherwise. In each regression specification the main explanatory variable is the interaction of the key variable with the recession indicator. The recession indicator takes a value of one in months classified as recession months by the NBER Business Cycle Dating Committee. The first key variable (specification (1)) is a follower indicator, which takes a value of one if in the past five years a given firm has announced a layoff in a 5-day window following a large firm layoff (including the day of the large layoff). Similarly, the counterfactual follower indicator (specification (4)) takes a value of one if in the past five years a given firm has announced a layoff in a 5-day window prior a large firm layoff excluding the day of the large layoff). All the specifications include month fixed effects, firm fixed effects, and firm level controls and the interaction of each of these controls with the recession indicator. The firm level controls include Earnings-Price ratio, Book-to-Market Ratio, Market Capitalization, Leverage Ratio, Years since IPO, which are all obtained from COMPUSTAT. Since the month fixed effects absorb all variation over time, the effect of a recession indicator is not separately identified in these specifications. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Sample		
Dependent Variable	Layoff Indicator (1)	Layoff Indicator (2)
Past 5 year Follower Indicator	0.0135*** (0.0035)	
Past 5 year Counterfactual Follower Indicator		0.0120*** (0.0036)
Recession × Past 5 year Follower Indicator	0.0314*** (0.0085)	
Recession × Past 5 year Counterfactual Follower Indicator		0.0041 (0.0095)
Mean	0.0722	0.0722
Std. Dev	0.2587	0.2587
Firm Fixed Effects	✓	✓
Month Fixed Effects	✓	✓
Firm Controls and Recession Interaction	✓	✓
Observations	70170	70170

2.8 Alternate Explanations of Layoff Behavior

The results in Sections 5 and 6 are consistent with the predictions of the model of strategic corporate layoffs; however, several alternative theories also predict the temporal clustering of both mass layoffs and announcements of mass layoffs. In this section, we discuss whether these alternate theories are partially or fully consistent with the broad set of results presented so far. The leading alternative theories focus on common shocks, compassionate CEOs, management learning from other firms, and market inattention. Sections 8.1-8.4 discuss these alternative mechanisms and explore whether their implications match the empirical results. Notably, we do not seek to reject these alternate explanations; rather, we argue that none of them can explain the full range of results in the previous sections. We see this as strong support for the conclusion that reputation management in the context of financial markets plays a significant role in determining the layoff behavior of large firms.

2.8.1 Common Shocks

The first alternative explanation for the patterns in Sections 5 and 6 is rooted in common shocks. If an aggregate shock hits a large subset of firms simultaneously, this will lead them to announce layoffs within a short period of time. A simple model with such common shocks will generate temporal clustering of layoff announcements, and also that of actual layoffs. A more sophisticated model of common shocks may generate excess sensitivity of firms with certain characteristics to these common shocks. If these firm characteristics are correlated with the structure of executive compensation or CEO tenure, we would not only observe temporal clustering of layoffs, but also that firms with greater equity-linked compensation or short-tenured CEOs are more sensitive to the common shocks.

While common shocks are certainly part of the story, the results in Figure 2.5 provide suggestive evidence against the common shock theory at a daily frequency. If either the crude or the more-sophisticated version of the common shock theory were true, we should observe responses in layoff announcements both before and after layoff announcements by large firms, both in the aggregate and within industries. However, the results demonstrate a stark asymmetry in the dynamic response of layoff announcements of Fortune 500 firms.

In the business days leading up to a large firm announcement, the response of layoff announcements of the other firms is flat and close to zero. By contrast, it jumps up on the day of the large firm announcement, and gradually returns to zero in the next 4-5 business days. Additionally, in the results of Table 7 we find that firms that announce layoffs within the five days following a large firm announcement are more likely to have greater equity-linked compensation and short-tenured CEOs. Conversely, no such association was found among the firms that lay off in the five days prior to a large firm's layoff announcement.

Over the longer horizons explored in our business cycle frequency data, our matching estimator results (based on size and four-digit industry) suggest a differential sensitivity of public firms' layoff behavior in response to recessions, compared to the behavior of matched private firms. Neither version of the common shock theory offers a clear prediction on the differential sensitivity of public and private firms, indicating that common shocks alone cannot explain the full range of our results.

2.8.2 Compassionate CEOs

Another mechanism that may generate the observed layoff behavior is that CEOs are compassionate and care about their labor force. This interpretation is compatible with the analysis in our model, but shifts the context of reputation from financial markets to the firm's employees. The degree of reputation concerns would therefore reflect factors such as altruistic motives or the CEO having strong ties to the labor force. Such motivations may lead the CEO to be biased against engaging in layoffs, and delay their layoffs until absolutely necessary. Consequently, CEOs with such characteristics will appear to pursue a cyclical layoff policy, or announce layoff announcements after other large firms have announced a layoff. Moreover, it is quite reasonable to expect that there may be strong correlations between a CEO's level of compassion for his employees, and his tenure and compensation structure.

Testing the general validity of this mechanism is difficult because the level of compassion of a CEO may manifest itself in a number of different ways. We propose the following narrow test to identify the strength of such a mechanism in explaining our observed empirical patterns. If the CEO has spent many years at the firm (greater than 10 or 15 years)

before being appointed its CEO (“home-grown CEOs”), then he is more likely to be compassionate. Conversely, CEOs that are externally-recruited or did not spend many years at the firm before being appointed to head it are less likely to be compassionate towards their labor force. We therefore evaluate the viability of this mechanism by testing its ability to explain the pattern of strategic firms having a greater sensitivity of layoff propensity to recessions.

We report the results of these tests in Appendix Table A4. The results suggest that home-grown CEOs who have been with the firm for many years are less likely to announce layoffs in general (although the point-estimates are imprecisely estimated). However, we find no effect of being home-grown on the cyclical nature of layoff announcements. Therefore, we conclude that though compassionate CEOs may alter the firm’s baseline layoff propensity, we find no evidence for this mechanism affecting layoff behavior differently over the business cycle. Thus, we cannot appeal to this mechanism in explaining the cyclical layoff behavior of the strategic firms in our analysis.

2.8.3 Market Inattention

Another alternate mechanism that can lead to ‘leader-follower’ behavior and potentially cyclical layoff policies is that firms are relying on market inattention to hide behind bad news. There are two versions of this alternate theory. The first relies on the market’s underreaction to information caused by limited attention of market participants (Dellavigna and Pollet (2008)). In this version, there are certain days, e.g. Fridays, on which the market participants pay less attention to news, and therefore firms choose to release negative information on such days so as to reduce the adverse reputational effect. Thus, we would expect to observe firms clustering their layoff announcements around certain dates, but this would not be driven by interactions between firms. Rather, it would be the direct result of firms responding to common external drivers of market inattention.

The second version of the market inattention theory is that firms are following large firm layoff announcements because it allows their news article to be pushed to the back pages of the newspapers (or analogously gain less prominence in television news or other media). If market participants have some information processing cost, they are less likely to

chance upon this negative news, allowing the firms to release negative news in a relatively ‘concealed’ manner.

The high-frequency event study results presented in Figure 2.5 already controls for day of week and calendar month to control for predictable market inattention. In addition, the regressions also control for whether the daily observation occurs within a week (before or after) the firm’s scheduled earnings announcement date. Correspondingly, the leader-follower behavior observed in Panels A and C of Figure 2.5 stems from mechanisms other than those suggested by the first version the market inattention theory. As for the second version, we refer to the results of Panel D. If firms were trying to hide behind other negative news, we should find the same mechanism to hold after days of major negative non-economic news (e.g. Hurricane Katrina). The results presented in the last panel, however, illustrate that there is no systematic change in layoff announcement behavior either before or after such major negative non-economic news. In addition, we separately conduct an analysis of the page of the Wall Street Journal on which each layoff announcement was originally reported. We find no significant difference in the placement of layoff announcement coverage on days of layoff announcements by large firms, when compared to other days. While it is possible that market inattention may play a role in determining the timing of layoff announcements, we are unable to find any evidence for this in our analysis, and cannot appeal to this mechanism to explain our results at either the daily or the business cycle frequency.

2.8.4 Learning from Other Firms

A final alternate mechanism that may lead to ‘leader-follower’ behavior is that managers are uncertain about the state of the aggregate economy, and they are waiting to receive a signal from the actions of the largest firms in the economy. By virtue of being larger, the managers of the largest firms may have better information about the aggregate state. Consequently, managers learn about the aggregate state from the performance of large firms, and respond to layoff announcements by large firms with layoffs of their own. This mechanism would also predict that short-tenured CEOs would be more likely to react to the announcements of large firms, as they are more likely to be inexperienced, and thus

more reliant on learning from other managers.

Despite its intuitive appeal, this mechanism cannot account for the differences we observe between public and private firm layoff behavior. If the ‘learning-from-others’ theory is the primary driver of layoff policy, we should observe little difference between public and private firms, particularly when matching on size and industry. We would expect all firms within an industry to face similar market conditions regardless of their ownership status, so the optimal response to learning about changes in market conditions should be identical across the two groups. We therefore conclude that a learning-based mechanism cannot account for the variation we observe along this dimension, indicating that we continue to find a need to appeal to reputation concerns to explain the full range of our empirical findings.

2.8.5 Alternate Mechanisms: Taking Stock

The key conclusion of this section is *not* that the alternate mechanisms discussed here play *no* role in the high-frequency clustering of layoff announcements or the cyclicity of layoffs at the business-cycle level. Instead, we conclude that the main results of this paper cannot be fully explained solely by these alternate mechanisms. Instead, we argue that reputation concerns in the context of financial markets represent the most salient explanation for the patterns we observe.

2.9 Conclusion

In this paper we documented that there is excess clustering of layoff announcements at weekly (and in some cases daily) horizons. We interpret this clustering of announcements within a theoretical framework in which managers delay layoffs during good economic states to avoid damaging the market’s perception of their ability. We test the implications of our model at both daily frequencies and business cycle frequency using two novel datasets. Based on a matching estimator that matches each public firm to a private firm based on size and 4-digit industry level, we find that the propensity to layoff of private firms increases by roughly 2.4 percentage points in recession months. By contrast, public firms’ propensity to layoff in recessions increases by an additional 2.4-3.2 percentage points. That is, public

firms are twice as likely to engage in a mass layoff in a recession month compared to their matched private counterpart. In a range of tests we show that these differences are not being driven by public-private differences in lifecycle effects, leverage, size of workforce, or on our matching criteria. Within our sample of public firms, we find that firms that are predicted to be more strategic are also the ones which are more likely to engage in a mass layoff during recessions. Our results, therefore, suggest that the difference in reputation management is an important driver of the observed differences in the cyclicalities of layoffs between public and private firms.

At the daily frequency, we also find support for our model. We show that a large firm announcement (i.e. largest 20 firms based on past year's revenue) is associated with future layoffs by other Fortune 500 firms, but not with past layoffs. We find that this effect is twice as strong if the largest 20 firm is in the same industry as the follower firm. For our sample of privately held firms we find no such clustering behavior either before or after the large firm layoff announcement. Moreover, when we compare the characteristics of firms that lay off within 5 days after a large firm announcement ("followers") to those that layoff within 5 days before a large firm announcement ('counterfactual followers'), we find that the follower firms are much more likely to engage in a mass layoff in subsequent recessions compared to the counterfactual firms. This link between the daily frequency reputation management behavior to cyclicalities of layoffs over the business cycle is strong evidence in support of the reputation management hypothesis of this paper.

Chapter 3

Credit Constraints in the Funding of Innovation: Theory and Evidence

3.1 Introduction

While the importance of innovation for macroeconomic growth has been an established idea in economics since the days of Joseph Schumpeter, it is only in the last two decades that the microeconomics of innovation have received significant attention. A strong literature has developed to study innovation at the corporate level, and there has been significant progress in understanding the impact of determinants such as intellectual property rights and workplace incentives (Murray et al 2009, Azoulay et al 2011). One of the key determinants of corporate innovation has been the role of finance; while there is a broad literature on the structure and mechanics of early-stage financing such as angel investors and venture capital, only a small number of papers have addressed the causal impact of finance on innovative activity¹. While the intersection of finance and innovation is not yet well-explored, this is not due to a lack of significance. Predicting the importance of this connection, Schumpeter defined capitalism as an “economy in which innovations are carried out by means of borrowed money.”

As both innovation and early-stage finance have grown in importance in recent decades,

¹In this small literature, I highlight the result of Kortum and Lerner (2000), who show that greater venture capital activity in a given industry leads to significantly higher rates of corporate patenting.

the primary challenge to researchers has been their interconnected nature: financial investment responds to innovative opportunities, and innovators simultaneously respond to the incentives created by their funding sources (Martinson et al 2005). Recent papers have worked to address this challenge by taking advantage of relatively exogenous shocks to conditions in financial markets and estimating the impact of these shocks on innovation in a sample of firms where some are more sensitive to the shock than others (Bernstein 2012, Townsend 2012). This paper pursues a complementary approach, focusing in local rather than global shocks to early-stage financing, and measuring impacts at the level of individual researchers. In particular, this paper focuses on the transition point between academic and private-sector innovation, and seeks to understand the interplay of finance and innovation in this setting.

The main prediction analyzed in this paper is that higher levels of funding availability push academic innovators to either transition to the private sector, or to pursue innovations which more closely resemble those done in the private sector. To formulate these predictions more precisely, I develop a model of credit constraints in multi-stage innovation, and show that an easing of financial constraints leads to an earlier equilibrium transition point from academia to the private sector. Further, I show that while looser constraints expand the set of viable innovations, the cyclical nature of financing offers an incentive for academic researchers to pursue shorter-horizon innovations which are closer to the transition point. The model therefore predicts that greater funding availability will lead to increases in transitions to the private sector and the quantity of innovation, but decreases in the time horizon and scope of applicability for these innovations.

I test the above predictions using the patent output of a sample of life science researchers from top US universities. In order to effectively estimate the causal effects of funding availability on innovation, I take advantage of reinvestment dynamics in venture capital, using three important dimensions of separation to identify exogenous variation in life science investment. The first dimension is based on the mechanics of reinvestment in the venture capital industry, where exits such as IPOs lead to a rise in fundraising, and the funds are deployed to new investments over a period of one to three years. The second dimension is based on separation between industries: I take advantage of the presence of orthogonal variation between IPOs in different industries to obtain a measure of

funding availability which is relatively uncorrelated with innovative opportunities. Most significantly, the third dimension on which I focus is that of local effects: because venture capital requires significant monitoring and the use of local information, I can incorporate cross-sectional variation in funding availability to implement a difference-in-differences approach, comparing high-VC and low-VC regions across “hot” and “cold” funding years. Using these sources of exogenous variation, I instrument for local investment in life-science ventures using lagged IPOs outside the life sciences at the local level.

Using the instrumental strategy described above, I estimate the impact of funding availability on innovative activity, testing the predictions from the theoretical analysis. I find that increased funding does lead to significantly higher levels of transitions to private-sector innovation, and higher patent production, particularly among academic innovators. Further, I find that while academics do tend to pursue innovations whose applications have a longer horizon and a broader scope than private-sector innovations. However, controlling for this baseline effect, I also find that greater funding leads to decreases in both the horizon and scope of application for the patents of academic innovators. These results support the conclusion that financing impacts not only the career trajectory and quantity of innovation for early-stage researchers, but that it also changes the nature of innovative activity by promoting shorter-horizon, narrower-scope projects. This implies that high levels of funding may impede the earliest research stages, where basic-science innovations and the exploration of new ideas is most likely to take place. The paper’s main findings therefore highlight a familiar tradeoff in a new context: much like the difference between academia and the private sector, the role of finance in early-stage innovation leads to greater focus and increased quantities of innovation while reducing exploration and long-horizon projects.

The paper is organized as follows. Section 2 develops the theoretical model of credit constraints in multi-stage innovation and formulates testable predictions. Section 3 outlines the framework of the two-stage empirical strategy, and argues that it estimates the causal effects of funding on innovation. Section 4 describes the data sources and the construction of measures tracking both funding and innovation. Section 5 presents the empirical results and Section 6 concludes.

3.2 Theoretical Model

The theoretical analysis looks at the impact of funding availability on the process of multi-stage innovation, building on the framework developed by Aghion, Dewatripont and Stein (2008, henceforth ADS). The following sections extend this framework along two important dimensions. The first section adds a credit constraint to capture the effect of funding availability on both academic and private-sector research. The second introduces business cycles by allowing funding availability to vary across time. The main results from these extensions fall along two primary margins: along the “extensive” margin, greater funding availability results in an earlier transition from academia to the private sector. Along the “intensive” margin, greater funding leads researchers in academia to prefer research lines of shorter durations.

3.2.1 The Impact of Credit Constraints

This section describes the addition of credit constraints to the ADS framework. The main focus of the following analysis is the impact of credit constraints on the optimal transition point from academia to the private sector. I maintain the generality of the ADS framework: research lines start with an initial idea I_0 , which progresses through stages of refinement I_1 , I_2 , and so on, until the final idea I_k generates a marketable product with value V . There are two types of agents in the model: entrepreneurs who manage the development of research lines, and workers who provide (skilled) labor as an input. Entrepreneurs choose between developing the research line in academia or in the private sector, while workers choose between “practical” or “alternative” research strategies. Entrepreneurs maximize (expected) profits, while workers value their wages and the ability to pursue their preferred strategy.

The practical strategy has a higher rate of success, causing the research line advances to the subsequent stage with probability p . Private-sector research is defined by the exclusive use of this strategy, and is thus more effective than academia. In academia, the alternative strategy is sometimes employed by the workers, and the probability of advancing to the subsequent stage is only αp , with $\alpha < 1$. The tradeoff between these strategies is one of

cost: wages and related costs in the private sector are w_p , while the equivalent costs in academia are only w_a , with $w_p > w_a$. In the ADS framework, the difference in costs is driven solely through wages paid to workers, who demand a wage premium in exchange for the loss of the freedom to choose their preferred strategy.² The difference in costs $\Delta w \equiv w_p - w_a$ can be interpreted as also capturing any costs of enforcing the use of the practical strategy and any difference in capital intensity given the greater level of focus in the private sector.³

In order to finance the cost w of a given research stage, the entrepreneur must borrow against the project's expected payoff. When borrowing, all research organizations face the same constraint: they can only borrow up to a fraction of the project's expected payoff.⁴ The functional form I use for credit constraints is founded on the analysis of Holmstrom and Tirole (1997), where entrepreneurs cannot pledge the full amount of their expected income because this would destroy their incentives to avoid shirking. Specifically, I focus on the case where entrepreneurs can only borrow a fraction μ of their expected income.⁵ I express the credit constraints for stage i as: $\mu p \Pi_{i+1} > w_p$ in the private sector, and $\mu \alpha p \Pi_{i+1} > w_a$ in academia, with $\mu \in [0, 1]$.

Intuitively, setting $\mu = 1$ corresponds to full borrowing ability: research projects with positive expected payoffs can always be funded. At this level of borrowing ability, the

²Stern (2004) estimates the private-sector wage premium to be approximately 20-30%.

³In the interest of focusing on the most intuitive form of the model, I do not analyze hybrid research environments or the impact of physical capital. While hybrid structures might allow for the presence of incentives in academia, or a willingness to grant workers freedom to pursue their preferred strategy in the private sector, the presence of fixed costs of enforcement, such as a more hierarchical management structure, would make such intermediate forms less appealing. Physical capital is certainly an important input for scientific research, but modeling it explicitly does not add any intuition to the model. Notably, its effect can be captured by its impact on the success probabilities p and αp , and the total research costs w_p and w_a in the private sector and academia, respectively.

⁴It is conceivable borrowing constraints may differ between academia and the private sector. Academic institutions have access to federal grant funding and tax-preferred donations, and can issue tax-free debt under certain circumstances (Gentry 2002, Glaeser 2002). Because the bulk of the analysis focuses on the impact of the constraint on private-sector research, the results continue to hold if academia faces less binding constraints than the private sector.

⁵Note that if workers' wages and expenditures on enforcement could be financed through equity instead of wages, the same constraints would apply as long as the required returns demanded by workers and enforcement providers is equal to the required return of investors.

results from ADS are entirely unchanged because the constraint never binds. Specifically, the optimal transition point would be the first stage i^* to satisfy $(1 - \alpha)p\Pi_{i^*+1} > \Delta w$. When credit constraints are introduced, there are two possible points at which they may begin to bind. One possibility is that the very first stage of the line becomes impossible, meaning that the research path is never explored, and no research is observed. The other possibility offers more potential for observation: the first private-sector stage becomes constrained, and this stage is instead performed in academia. These results are summarized in the following propositions.

Proposition 3.1. *If stage i satisfies the borrowing constraint under research setting $s \in \{a, p\}$, then all later stages will also satisfy the borrowing constraint under the same research setting.*

Proposition 3.2. *If stage i , absent borrowing constraints, is optimally performed in academia, and if the private sector satisfies the borrowing constraint for stage i , then academia will also satisfy the borrowing constraint for stage i .*

Proposition 3.3. *A sufficient condition for academia to satisfy the borrowing constraint in at least some cases where the private sector does not is given by: $\frac{\alpha}{1-\alpha}\Delta w > w_a$.*

Proof. See appendix. □

From Proposition 1, it follows that as credit constraints tighten, the two stages which might be affected first are the first academic stage at the start of the line, and the first private-sector stage at the optimal unconstrained transition point. Proposition 2 indicates that if the first academic stage becomes constrained, the private sector will also be constrained in that stage; thus, the research line will not be initiated. Proposition 3 demonstrates that under the same condition for the viability of academic research in the unconstrained model, there is scope for academia to replace the private sector as credit constraints tighten.

Figure 3.1: Equilibrium Selection

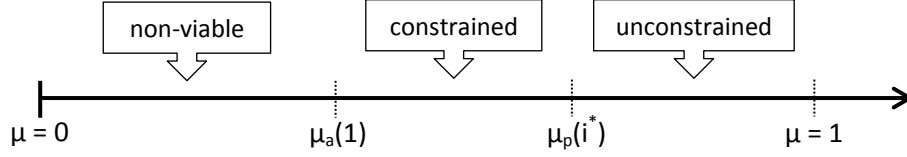


Figure 3.1 highlights the impact of credit constraints on a given research line. On the right-most side, near the point where $\mu = 1$, the borrowing limits do not impose any restrictions and research proceeds according to the ADS model. I label this the “unconstrained” equilibrium. As borrowing limits tighten, the constraints eventually begin to bind, at the point where $\mu = \mu_p(i^*)$, which satisfies $\mu_p(i^*)p\Pi_{i^*+1} = w_p$. At this point, the first private-sector stage becomes constrained, and the equilibrium becomes “constrained” - specifically, the research stage i^* , which would optimally be performed in the private sector, is now performed in academia. As credit constraints tighten further, more private-sector stages will transition into academia. This process continues until the borrowing multiplier falls to the point where $\mu = \mu_a(1)$, which satisfies $\mu_a(1)\alpha p\Pi_1 = w_a$. At this point, the first stage in academia no longer satisfies the borrowing constraint, and the line cannot begin.⁶

Once credit constraints become binding, the transition point from academia to the private sector will be defined not by the optimality conditions described in ADS, but by the ability to satisfy the borrowing constraint. Recall that in ADS, the first-best optimal transition into the private sector occurs at stage i^* , which is the smallest value of i to satisfy: $p\Pi_{i+1} > z$. By contrast, when credit constraints are binding, the transition point will be less efficient than the first-best. Instead, there will be a second-best equilibrium, with the move to the private sector occurring at stage \hat{i} , which is the smallest value of i to satisfy:

$$\mu p\Pi_{i+1} > R + (1 - \alpha)z$$

Combining these conditions, it follows that the transition point into the private sector will

⁶Note that Π_1 is also a function of μ in the constrained equilibrium, because shifting stages from the private sector to academia in response to borrowing constraints is sub-optimal, and reduces the profitability of the entire research line. However, in determining whether there is scope for the constrained equilibrium in a given research line, we need only compare deviations from the unconstrained equilibrium. Thus, we can conclude that the constrained equilibrium will exist for some non-empty range of μ if $\frac{\alpha\Pi_1}{w_a} > \frac{\Pi_{i^*+1}}{w_p}$.

occur at stage $\max\{i^*, \hat{i}\}$. From this, we can directly calculate comparative statics on the transition point from academia to the private sector.

Proposition 3.4. *Holding fixed the number of stages k in a research line, the optimal transition to the private sector occurs earlier if (i) V is greater, (ii) z is smaller, and (iii) R is smaller, under both the constrained and unconstrained equilibria. Further, when credit constraints are binding, the transition to the private sector will occur earlier if (iv) μ is greater.*

Proof. See appendix. □

The main testable result in this analysis is the comparative static with respect to μ : an increase in borrowing ability shifts the transition point to an earlier stage in any given research line.⁷ Recall that μ stems from the Holmstrom-Tirole (1997) framework, where it is a function of both the fraction of expected earnings which the entrepreneur can pledge, and the required rate of return demanded by investors. While the pledgable fraction is determined by the details of shirking opportunities and is likely fixed across time, the required rate of return is highly variable. Thus, whenever private-sector funding of research is particularly plentiful, and investors are willing to accept lower returns, research projects and their associated entrepreneurs will be able to borrow greater sums against expected income, and this will make them more likely to transition out of academia and into the private sector.

The shift in transition point is an effect along the extensive margin: it is a direct transition from one research environment to another. However, an analysis based on comparative statics makes no predictions about research stages not near the point of transition - entrepreneurs who remain in academia are predicted to be unresponsive to changes in funding. To relax this assumption, the next section focuses on dynamics: it introduces a Markov process for funding availability, and analyzes the impact this has on research project choice within academia.

⁷Because the above analysis maintains the generality of ADS framework, it is fully amenable to the extensions presented there, including alternative specifications for project success probabilities, and the addition of branching research lines.

3.2.2 Funding Cyclicity

In this section, I introduce dynamics into the above analysis by allowing for variation across time for the borrowing ability μ of entrepreneurs. For simplicity, I focus on the case where the funding level determines whether the last stage or a research line is performed in academia or the private sector, and evaluate the impact of this shift on the relative profitability of earlier stages.⁸

I begin with an outline of the model's timing at each stage of the research process: At the start of any given stage in the research line, the first event is the realization of the borrowing limit which will apply for the remainder of the stage. The entrepreneur then decides whether to pursue the research line under academia or the private sector. Next, a worker is hired and a wage is determined within the labor market. Finally, the choice between the practical and alternative strategies takes place, the project outcome is realized, and the stage completes.

The borrowing limit follows a two-state Markov process, with full borrowing ability in the high-funding-availability state, and a borrowing limit of $\mu < 1$ in the low-funding-availability state. The degree of persistence for these states across time is denoted by the correlation measure $\rho \in [0, 1]$, such that the Markov transition matrix is:

$$\begin{bmatrix} \frac{1+\rho}{2} & \frac{1-\rho}{2} \\ \frac{1-\rho}{2} & \frac{1+\rho}{2} \end{bmatrix}$$

Note that the above matrix is symmetric, and a value of $\rho = 0$ gives a memoryless process while $\rho = 1$ indicates perfect persistence of the funding state across time. As shown in the previous section, setting the borrowing multiplier equal to one in the high-funding state guarantees that the transition point will be chosen to maximize the value of the line. In the constrained state, with $\mu < 1$, the choice of research environment will be limited by the ability to borrow sufficient funds to support up-front costs. The following assumptions focus the analysis on the range of research projects whose transition point depends on the level of funding availability.

⁸If researchers care about expected citation rates as well as profitability, the impact of funding will be amplified. For a detailed theoretical analysis of the tradeoff between wages and expected citation rates, see Gans, Murray, and Stern (2011).

Assumption 3.1. *Suppose that the following conditions hold for a given research line:*

$$A1.1: (1 - \alpha)pV > \Delta w$$

$$A1.2: \mu pV < w_p$$

The first inequality guarantees that the last stage will occur in the private sector if the funding state is high. Similarly, the second inequality focuses on the case where the borrowing limit μ in the constrained state is sufficiently low to push the final stage k from the private sector into academia. Importantly, the impact of this shift will be a change in expected payoff, not only for the final stage but also for stages which precede it.

In the final stage, the change in payoff can be calculated directly: research in the private sector earns $E(\pi_k^p) = pV - w_p$, while research in academia earns $E(\pi_k^a) = \alpha pV - w_a$, for a reduction in payoff of $\Delta\Pi_k = (1 - \alpha)pV - \Delta w$.⁹ A shift in the payoff of the final stage will also lead to a shift in expected payoffs for earlier stages. However, because of the possibility of the funding-availability state changing at the start of each stage, this shift in payoffs is discounted more strongly than payoffs which depend solely on the success of the research line. This intuition is formalized in Proposition 5 below:

Proposition 3.5. *When the funding-availability state is markovian with correlation ρ , a research line which shifts from the private sector to academia at stage k due to funding constraints will experience the following shift in payoffs in stage $k - i$:*

$$\Delta\Pi_{k-i} = (\rho\alpha p)^i \Delta\Pi_k$$

Proof. See appendix. □

From the above proposition, it follows directly that the impact of the funding state diminishes as one moves to stages farther from the transition point to the private sector. Because of this differential impact for research paths at different stages of development, it is possible for the funding-availability state to have an impact on project choice within academia, in addition to the impact on the transition point to the private sector described in the previous section.

⁹Note that this analysis applies to any research line which has at least one stage optimally performed in the private sector - one would simply replace V with the expected payoff from the remaining stages of the research line, and the analysis would proceed symmetrically.

To analyze the impact of funding availability on project choice *within* academia, I frame the analysis in terms of a reversal of preference. Intuitively, Proposition 5 states that the closer a project is to the transition point into the private sector, the more important the funding state becomes. As a result, an entrepreneur will prefer to pursue shorter-horizon lines when funding availability is high, and longer-horizon lines when it is low. While this reversal is possible across a broad range of theoretical settings, I will focus on a simple baseline case to highlight the impact of credit constraint cyclicity on project choice within academia.

Suppose that an entrepreneur has two research lines available to him: a short line $i + 1$ stages away from completion, and a long line which is $i + 2$ stages away. The lines are identical in all other respects - they require researchers from the same labor pool (leading to equal values of α , w_a , and w_p), have the same success probability p at each stage, and have the same final payoff V . Both lines satisfy the conditions of Assumption 1 in their final stage, meaning that it will take place in the private sector if the funding state is high, and in academia if the funding state is low.¹⁰ To focus on the central intuition, I further assume that the value of performing the final stage in academia is equal to zero. While I present the full equilibrium characterization in the appendix, Proposition 6 describes the main result.

Proposition 3.6. *Suppose an entrepreneur must choose between a short research line i stages from transition, and a long research line $i + 1$ stages from transition. If per-stage wages are small relative to project value, sufficient conditions for the existence of preference reversal in academia are:*

- $\alpha p > \frac{i}{i+1}$
- $\rho \in \left(\frac{2i}{\alpha p(i+1)} - 1, 1 \right)$

Under these above conditions, the short research line will be preferred in the high-funding-availability state, and the long research line will be preferred in the low-funding-availability state.

¹⁰As with Proposition 5, this analysis generalizes to all research lines which have at least one stage optimally performed in the private sector.

Proof. See appendix. □

Corollary 3.1. *In the case where the shorter research line is one stage away from transition, i.e. $i = 1$, the following conditions are both necessary and sufficient for preference reversal:*

- $\alpha p > \frac{1}{2}$
- $\rho \in \left(\frac{1-\alpha p}{\alpha p}, 1 \right)$

Proof. See appendix. □

In the above proposition, the main intuition is that the possibility of funding state deterioration while in the high state increases the effective discount rate during the high-funding state. By contrast, the likelihood of funding state improvement in the low state decreases the effective discount rate. This wedge between effective discount rates can be large enough to overcome the disadvantages that longer lines face, namely, reduced chances of ultimate completion and higher labor costs. The conditions above illustrate this directly: αp must be sufficiently high, meaning that an additional academic stage must not lead to a large drop in the value of the research line. At the same time, ρ must be large so that the wedge between the discount rates in the high- and low-funding states is large enough to overcome the costs of an additional academic stage.

Importantly, the above analysis should not be interpreted as an argument that entrepreneurs in research fields are motivated solely by financial incentives. Rather, the primary conclusion is that as long as financial incentives play a role in entrepreneurs' decisions, greater funding availability will push them toward shorter-duration projects.

By introducing funding state dynamics, this section argues that the impact of funding along the extensive margin will also have a recursive impact on researchers remaining in academia. Because funding availability reduces expected payoffs when lines become constrained, the current funding state also impacts the decisions of researchers at earlier stages. This leads to a new effect, occurring on the intensive margin: academic researchers will prefer shorter research lines during periods of greater funding availability.

3.2.3 Testable Predictions

The model described above introduces credit constraints and cyclicalities into the model of multi-stage research. This approach leads to predictions for the impact of funding availability on the types of projects researchers choose to pursue. The model identifies two main margins along which this effect operates. Along the extensive margin, greater funding availability pushes a greater share of research stages to occur in the private sector, rather than academia. Along the intensive margin, the model predicts that increased funding shifts academics toward shorter research lines. While not modeled explicitly in this analysis, previous literature has shown that a focus on shorter research lines is also associated with projects characterized by lower levels of exploration and collaboration, and projects with a narrower range of applicability (Nelson 1959, Scotchmer 1991, Murray et al 2009).

In setting the stage for the empirical analysis in the following sections, it is useful to set forth the testable predictions of the model.

- i. Research performed in academia will be earlier-stage than research performed in the private sector.
- ii. Greater funding availability will lead to transitions from academia to the private sector.
- iii. By easing constraints, greater funding will lead more research projects to be viable.
- iv. Within academia, more funding will shift the distribution of research projects toward later stages.

The first prediction is based on the original framework in ADS (2008), where academia optimally precedes the private sector. The second prediction is based on the analysis along the extensive margin: as the transition point shifts earlier in response to greater funding availability, researchers will find that it is optimal to move from academia to the private sector. In addition to shifting the transition point between academia and the private sector, greater funding availability will also ease potential constraints at the first stage of academia, which determine whether or not a research line is viable. By easing this initial constraint, greater funding will expand the set of feasible projects and has the potential to increase the

quantity of innovation. The last prediction stems from the impact along the intensive margin: researchers who remain in academia have an incentive to shift to later-stage research to take advantage of the greater benefits of reaching the point of commercialization while funding availability is high.

In the next section describes the empirical strategy which seeks to test the broad predictions outlined above, and expands on the details of measuring concepts such as career transitions, early- and late-stage research, collaboration, and project scope.

3.3 Empirical Strategy

This section describes the empirical strategy used to test the theoretical predictions in the previous section. The primary challenge in examining the impact of funding availability on the process of innovation is that of endogeneity. Specifically, one would expect that funding responds the changes in research and investment opportunities, leading to spurious relationships in OLS specifications. In addition to this, one would expect a wide range of variation in treatment effects, as researchers at different stages of the innovative process will have heterogeneous responses to similar changes in funding availability. To address these challenges, I focus on an instrumental variables approach within a panel setting, tracking the output of a sample of individual researchers over time.

To obtain an effective instrument for funding availability, I require a measure that satisfies the exclusion restriction: its relationship with innovation must work only through funding, without any correlation with the impact of uncontrolled determinants. While a full set of fixed effects and a broad range of additional controls helps to limit the scope of uncontrolled determinants, a number of challenges remain even after including these variables. To achieve a sufficient level of exclusion, I focus on three dimensions of separation between funding and innovative output. Described below, these dimensions can be summarized as the venture capital cycle, cross-industry funding impact, and local reinvestment effects.

3.3.1 The Venture Capital Cycle

The first dimension of separation to which I appeal is that of the cyclical nature of venture capital. Specifically, the industry operates under a model which begins with the funding of early-stage ventures, develops them into more-mature companies, and then exits through an IPO to the public, or a sale to an acquiring firm. Importantly, when such an exit occurs, the venture capital firm and its investors seek to re-invest the proceeds into new ventures. This search for new investments in the wake of recent exits would lead to greater funding availability for early-stage entrepreneurs seeking financing. Because the exits occur at the IPO stage of a firm's development, this establishes a separation between the investment opportunities of the mature firms involved in the exit, and the young firms seeking early-stage financing.

In practical terms, the age of the average firm going through a venture-backed IPO is just over six years (Gompers 1996), indicating that there is a significant temporal separation between entering and exiting firms. This temporal separation is also likely to correspond to a separation in terms of investment opportunities: factors such as business opportunities, discount rates, and risk premia offer only loose predictability at these time horizons, and most of this predictability can be controlled using observable variables. Specifically, if IPOs are driven by a combination of interest rates, investor risk preferences, and growth opportunities which reward investment at a particular moment in time, these factors would have little influence on the value of a new venture. Gompers and Lerner (1999) show evidence for this when they show that while capital inflows into the venture capital industry lead to higher valuations (and thus lower required rates of return) for new ventures, these high valuations do not correspond to increased levels of success for the newly-funded ventures in terms of the likelihood of going through or filing for an IPO.

In light of the above evidence, there seems to be a significant degree of separation between the determinants of venture capital exits, and the quality of investment opportunities available in the next funding cycle. In this view, IPOs are driven by conditions in financial markets, which have no direct impact on the viability of early-stage ventures and which are unlikely to persist on the timescales required for these ventures to benefit when they reach the point of going public. This suggests that an analysis based on reinvestment and taking

advantage of lagged measures of funding has the potential to capture causal links between funding and innovative outcomes.¹¹ While previous work has often looked to fundraising and capital inflows in measuring the availability of new funds, an approach based on post-exit reinvestment adds an additional layer of separation. Fundraising may capture new funds entering the world of venture capital, possibly in response to new investment opportunities, while reinvestment is a much more mechanical relationship which simply assumes investors seek to continue their pre-exit investment allocation. To do so, they would need to reinvest the proceeds of the exit. In some cases, this occurs immediately through capital recycling provisions, while in others the sensitivity of fundraising to past performance leads to the reinvestment of proceeds into the next fund being raised by the venture capital firm¹². The main purpose of focusing on reinvestment is that it excludes channels of impact that are driven by shocks specific to the value of early-stage ventures: the link to exiting firms means that we need only worry about investment opportunity shocks which are common to both early-stage and mature firms.

By focusing on this mechanical link between exits and new investments, there is a greater degree of separation between investment opportunities and funding availability, allowing for a stronger case in favor of the exclusion restriction. Nevertheless, it is reasonable to argue that exits and investments in the same industry are likely to share significant links in their potential profitability: both established computer hardware manufacturers and startup software firms are likely to benefit from events such as the internet boom. To address this channel, the next section adds a second dimension of separation by focusing on cross-industry effects in funding availability.

3.3.2 Cross-Industry Funding

A second useful dimension of separation between funding availability and investment opportunities stems from taking advantage of variation across industries. Thus, in addition to using IPOs to predict investment in new ventures, one can focus on using IPOs in

¹¹For an example of this type of approach, see Nanda and Rhodes-Kropf (2012).

¹²See Rossa and Tracy (2007) for details on capital recycling provisions, and Kaplan and Schoar (2005) for the links between past performance and follow-on fundraising.

one industry to predict investment in another. This approach expands the set of excluded channels of impact: it now rules out industry-specific shocks to profitability or investment opportunities, in addition to ruling out shocks that are specific to either only mature or only early-stage firms. For this dimension to be useful in establishing a causal link, it must be the case that different industries have relatively independent drivers of investment opportunities, conditional on observable controls such as interest rates and economic conditions. While it is true that there is significant overlap across industries for inputs such as corporate real estate, support staff, and general-purpose physical capital, these are provided through relatively competitive markets; by contrast, the main sources of variation in the value of innovative industries tend to be idiosyncratic rather than systemic (Hoberg & Phillips 2010). As a result, changes in profitability one industry are unlikely to have a significant and persistent relationship with profitability in other industries, after controlling for observable factors. This means that when equity investors are willing to fund a wave of IPOs in response to opportunities in one industry, this is unlikely to correlate with investment opportunities in an unrelated field.

An approach based on cross-industry effects has offered important contributions in analyzing the impact of funding availability on investment by established firms. Lamont (1997) focuses on the non-oil subsidiaries of multi-segment oil companies, and finds that in response to the 1986 oil price decline, investment by these subsidiaries decreased significantly more than investment by the median firms in those industries. This highlights the fact that internal capital markets are often disconnected from external ones, indicating that agency costs and informational asymmetries can lead to an interdependence of projects in different industries if they are controlled by a single firm. These informational asymmetries and agency costs are likely to be even greater in the world of venture capital, where monitoring, learning, and moral hazard are integral to the challenges faced by investors (Chan 1983, Bergmann & Hege 1998). Thus, one may well expect that the performance of venture capital exits in one industry are likely to have an influence on funding availability for investments in unrelated industries. While my focus is on funding for early-stage ventures, Townsend (2011) has shown a cross-industry effect for follow-on financing in venture capital: non-IT ventures financed by firms with heavy IT exposure were much less likely to receive follow-on financing following the collapse of the internet bubble, despite

being observationally similar to non-IT ventures funded by venture firms with little or no IT exposure.

Based on this evidence, an empirical approach based on cross-industry effects is an effective way to improve the separation between funding availability and investment opportunities. By combining separation across the venture capital cycle with separation across industries, the range of potentially problematic variation is greatly reduced. Specifically, the relevant sources of concern must be comprised of shocks which impact both exiting firms and early-stage ventures, and which apply across multiple broad industries. While this approach excludes a wide range of problematic variation, it does not fully satisfy the exclusion restriction: even with a full set of controls based on observable factors, there are likely to be channels such as technological advances or shifts in human capital development that impact ventures across the range of industries and lifecycle stages. To address these forms of variation, I introduce the third dimension of separation by focusing on the impact of local effects in entrepreneurial finance.

3.3.3 Local Effects

The empirical strategy that I pursue depends in part on variation in the return required by investors in early-stage innovation. In the previous section, I noted evidence in past research for the idea that there are significant imperfections in capital markets, leading to variation in investment which is driven by financial considerations rather than investment opportunities. It is precisely this form of variation which is needed in order to satisfy the exclusion restriction for being a valid instrument. Many past studies have emphasized the time-series aspect of variation by focusing on shocks such as the collapse of the internet bubble or the start of pension fund investments in venture capital in 1979. This paper seeks to expand the scope of previous analysis by adopting a panel-based approach tracking variation not only over time, but also across geographic locations. This local dimension is particularly valuable in the study of venture capital due to the importance of personal relationships, local information, and monitoring, which are central to creating value (Sorenson and Stuart 2001). Because of the importance of local connections in identifying and developing early-stage ventures, one would expect that local funding plays a crucial role in

early-stage finance, and that there are often significant differences in the nature of funding in different locations (Powell et al 2001). As a result, the geographic dimension is an important source of variation which can add an additional layer of separation between funding availability and investment opportunities.

The geographical fragmentation of markets for entrepreneurial capital is driven by the nature of venture capital investment. Further, this source of variation is amplified by a significant home-bias effect: investors, particularly pension funds, tend to prefer funding home-state projects, and their below-average subsequent returns indicate that this preference is not driven by greater knowledge of local investment opportunities (Hochberg and Rauh 2011). The combined effects of local relationships for venture firms, and home bias for investors, means that cross-sectional variation provides a valuable dimension of separation between funding availability and investment opportunities at the local level. Specifically, following a locally-financed IPO, most of the proceeds will return to the local innovative economy because the venture capital firms backing the IPO have well-developed local relationships, and because home bias means investors have a strong tendency to re-invest locally. Cross-sectionally, this means that geographic regions which experience IPOs will have a higher level of expected investment than regions which do not, even if the regions and investment opportunities are identical in all other respects.

By adding the geographic dimension to the dimensions of industry and lifecycle stage, I rule out a broad range of sources of problematic variation linking funding availability and investment opportunities. Specifically, the only remaining challenges would be based on shocks which impact investment opportunities in both early-stage ventures and IPO activity, and span multiple industries, and which are local in nature. Further, because of the ability to include time- and location-based fixed effects in a panel regression setting, I can account for both economy-wide unobserved temporal variation and persistent unobserved differences across locations. As a result, the only remaining concern is location-specific, time-varying shocks which impact multiple industries and influence both IPO-stage companies and early-stage ventures. While this source of variation cannot be dismissed a priori, I offer a range of robustness tests in Section 5 to rule out any such variation as a driver behind the results. Combining these robustness tests with the fixed effects and the instrumental strategy described above, I argue that I have a valid instrument satisfying the

exclusion restriction: a measure of local funding availability which is driven by shifts in required returns rather than investment opportunities. As a result, my instrument will offer a source of variation which impacts innovative output solely through the channel of funding availability, and allow me to infer the causal links between funding and innovation.

3.3.4 Impact on Innovation

The previous sections have argued that by taking advantage of separation along the dimensions of lifecycle stage, industry, and geography, it is possible to construct a valid instrument to capture the impact of funding availability on innovative output, without including effects working through the channel of shocks to investment opportunities. In this section, I describe how this measure can be incorporated into a framework to effectively estimate the impact of funding on a range of measures of innovation. The primary focus in this section is to expand on the nature of the panel dataset structure and the treatment effects it captures. In an ideal world, one might hope to estimate the impact of funding on innovation by means of a controlled experiment, with random assignment of the treatment and control conditions. Since this is not possible in the real world, my analysis focuses on what is effectively a difference-in-differences approach by taking advantage of both time-series and cross-sectional variation in my panel framework. I structure my analysis at the level of the individual researcher, because this is the level at which entrepreneurial decisions and tradeoffs take place¹³.

When estimating the impact of funding on innovation, I focus on output by individual researchers and estimate how the level and nature of this output varies in response to funding availability. Importantly, I measure funding availability at the local level. In order for this approach to effectively estimate the relationship, it must be the case that researchers cannot readily change locations in response to funding availability. For this reason, I focus exclusively on researchers in the life sciences, as their work is performed in capital-intensive research labs which are very difficult to relocate. In the empirical analy-

¹³It is also possible to focus more broadly, at the geographic level, or more narrowly, at the level of specific innovations. However, both of these alternatives are susceptible to bias if there are changes in composition over time: the number of researchers working in a given location may change, as may the quantity of information contained in a given innovation output such as a patent or publication.

sis, I confirm that the rate of location-transfers is very low and is almost always limited to one location change in transferring researchers. Further, I perform robustness tests to confirm that my results are not driven by these transfers. Because the location of researchers is largely unchanging from year to year, I can take advantage of variation in funding availability across years and locations to estimate a form of difference-in-differences effect. Specifically, because my sample includes locations with both high and low levels of presence by the venture capital industry, and because I cover multiple waves of IPOs and venture capital cycles, I am able to use the low-VC-presence areas as effective controls and contrast them with high-VC-presence locations. This is a conservative approach, as all areas tend to experience a rise in venture capital activity during hot years. For this approach to be valid, it must be the case that researchers at different locations are similar in their baseline innovative ability. To address this concern, I focus on a sample composed of researchers from top US universities, selected based on surveys of quality in graduate programs¹⁴, and control for differences in rankings within the top-ranked sample. This sample of researchers comprises a relatively homogenous and geographically stable population, allowing for a valid estimate of the impact of funding availability at the local level. Combining this with my instrumental approach described in the preceding sections, I am able to construct estimates of the causal impact of funding on innovation.

3.3.5 Regression Specifications

In this section, I describe the estimated equations of the regression analysis. I do so in broad terms, and move to the specifics of measurement and variable construction in the next section. Because of concerns about endogeneity, I adopt a two-stage least-squares approach through the use of instrumental variables, as described earlier. In doing so, I take advantage of three dimensions of separation between funding and investment opportunities: lifecycle stage, industry, and geographic location. The main idea in the first stage is to show that despite the strong separation along these dimensions, there is nevertheless a significant relationship between IPOs in unrelated industries and local investment in early-

¹⁴The surveys were performed by the American Council on Education (ACE) and the National Research Council (NRC) and were performed approximately once per decade spanning the years from 1964 to 2006.

stage ventures in the life sciences. To establish this, I estimate equations of the following format, using observations for region r in year t :

$$\log(VC_{rt}^{LifeSci}) = \alpha * \log(IPO_{rt}^{NonLifeSci}) + \delta_r + \theta_t + Controls_{rt}$$

In the above specification, δ_r and θ_t correspond to region and year fixed effects, respectively. I use a log-based specification to focus on proportional shifts, in order to more effectively incorporate observations of different magnitudes when using regions and time periods with a wide range of venture capital activity. In addition, I include time-varying region-specific measures; specifically, I use the log of population and the log of annual economic output for each region, as well as the average of academic rankings for all top universities in the region. Importantly, I focus the analysis on regions containing at least one of the 30 schools in my sample, in order to best capture the variation in venture capital activity which pertains to regions with a strong presence of academic researchers.

In addition to these control variables, I also use a wide range of measures of venture capital to capture both exits as an explanatory variable, and investment as a dependent variable. For the right-hand-side variable, my baseline specification is to look at the real value of venture-backed IPOs outside the life sciences which occurred in region r during years $t - 1$ and $t - 2$. The choice of lags for the baseline case is based on a combination of anecdotal evidence on reinvestment timing in the venture capital industry, and a lag-based where I compare various time horizons and find that the prior two years offer the strongest predictive ability in explaining future investment. As robustness tests, I also look at the number of IPOs, and examine the impact of dis-aggregating IPO activity by broad industries. For measuring venture capital investment as the dependent variable in the first stage, I focus on the real-dollar value of local venture capital investment in the life sciences during year t . For robustness, I also look at investment in early-stage life-science ventures, and the number of investments rather than their real-dollar value. The primary prediction in the first stage is that the coefficient on α will be positive and significant, indicating that cross-industry reinvestment at the local level is an important determinant of future venture capital investment, and offering a viable instrument to evaluate the impact of funding on innovation while excluding channels related to shocks in investment opportunities.

In the second stage of the instrumental variables analysis, I use the fitted values from the first stage to estimate the causal impact on a range of measures of innovation. While I reserve a detailed list of these measures for the following section, two primary dimensions of interest are the quantity of patents produced by researchers, and the scope of those innovations. I track patent quantity at the level of individual researchers, and again use a log-based specification to capture the full range of variation across the set of researchers in my sample. Specifically, for researcher j at school s in year t , I estimate:

$$\log(Patents_{jt}) = \beta * \log(VC_{rt}) + \eta_j + \sigma_s + \theta_t + Controls_{st}$$

In the above specification, η_j , σ_s and θ_t correspond to researcher, school, and year fixed effects, respectively. Controls again include population and economic output at the regional level, as well as academic rank at the school level. In addition, I control for researcher tenure using a polynomial in years since the first observation of a researcher in the patent database. This specification applies to both the OLS and IV estimates, varying only by the substitution of actual and instrumented venture capital investment in the life sciences. Further, the specification applies to the full range of innovation measures I consider, including innovation quality and innovation scope. In addition, by jointly estimating equations for the production of academic and industry patents, I can derive the impact of funding availability on career transitions as researchers move toward or away from the private sector. In these equations, the coefficient of interest is β , and its predicted direction depends on both the attribute being considered, and whether the analysis is applied to academic or private-sector innovation, as described in the section on theoretical predictions.

The regression specifications above offer an opportunity to test the implications of the model presented in Section 2, and to provide important insight into the links between funding and innovation. In the following section, I describe the sources of my datasets and the variables and measures I construct to capture both funding availability and the level and nature of innovative output.

3.4 Data and Variables

3.4.1 Data Sources and Sample Selection

The empirical analysis of the relationship between funding availability and innovation brings together a wide range of disparate data sources. My starting point is to identify a sample of high-quality researchers with readily identifiable locations. To do so, I take advantage of a series of five surveys of academic quality among graduate programs in the life sciences, performed by the ACE and NRC and spanning the years from 1964 to 2006. Through these surveys, I identify thirty top academic research institutions across the US, and obtain information on their academic rankings across the time period above. Taking this sample of schools, I then obtain the list of all patents assigned to them from 1975 through 1999 using data from the NBER Patent Data Project¹⁵ From the patents, I identify inventors and their geographic locations, and arrive at my sample of researchers by focusing on those with *three or more* life-science patents linked to the 30 schools I track. I then add in all other life-science patents assigned to these researchers, and continue to track inventors' locations as listed on their patent applications. This gives the full set of patents I consider in my analysis, though I often focus on the set of patents linked to regions containing the 30 schools in my sample, or to researchers who remain in a single location for the duration of their careers. For each entry in this set of patents, I obtain further information on assignees, patent classes, claims, and patent citations, and the patent class distributions of both citing and cited patents through the extended NBER Patent Data Project, which runs from 1975 through 2006¹⁶. This range of information forms the foundation of my measures of innovation.

The remaining data sources I employ are focused on measuring funding availability and related economic indicators. All of my variables are collected at the geographical level of Metropolitan Statistical Areas, or MSAs, as defined by the US Office of Management and Budget, and tracked by the US Census Bureau. The simplest measures, directly from the

¹⁵These data are described in detail in Hall, Jaffe, and Trajtenberg (2001).

¹⁶This extended range of data is particularly helpful in tracking citations, as it takes multiple years before research based on a published patent can work its way through the patent application process.

Bureau of Economic Analysis, are population and economic output levels by MSA from 1969 to 2009. Next, I collect data on venture capital investment at the MSA level from Thomson Financial's Private Equity Investments Screener database. I collect information on all investments defined as "Venture Capital Deals," which is a broad measure covering deals at the venture stage¹⁷, and any non-venture-stage deals made by traditionally venture-focused firms. For these investments, I collect a range of information on the stage of the investment, the location of both the investing firm and the receiving company, the industry of the receiving company, and the value of the investment. In addition to investments, I also collect information on venture capital exits. These are divided between IPOs and acquisitions. For both, I again collect information on location, industry, and deal value, and connect exits to the database of venture capital investments. This set of variables enables me to construct the necessary measures of funding availability, which I match at the MSA-year level to the data on innovation described above.

In the following sub-sections, I offer detailed definitions of the key variables of my analysis, in order to set the stage for the regression analysis.

3.4.2 Regional Measures and Funding Availability

The first variables I consider are those that comprise the first stage of the instrumental-variables strategy in the empirical section: specifically, the variables that measure funding availability. The dependent variable is the primary variable of interest: $\log(VC_{rt}^{LifeSci})$, which is the natural log of the value of all venture-capital investment in life-science companies, occurring in region r during year t . In the Thomson Financial database, this measure is denominated in millions of dollars, rounded to the nearest \$10,000. Using CPI data, I convert values from all years to 2005 dollars. I then take the log of these values, first adding approximately \$200,000 (the first percentile of non-zero investments) to all observations in order to avoid undefined values when no investment was observed.

Next, I take a similar approach to the measure of venture-backed exits. I construct the variable $\log(IPO_{rt}^{NonLifeSci})$ as the natural log of the value of all venture-backed IPOs

¹⁷Thomson Financial's classification of venture stages consist of startup/seed, early-stage, expansion, and late-stage deals.

outside the life sciences undertaken by portfolio companies in region r in year t . I again scale all observations to 2005 dollars. In these observations, the first percentile of non-zero values is approximately \$5,000,000, so I add this value to all observations before taking the natural log to avoid undefined entries. I track this measure not only at the aggregation level of all industries outside the life sciences, but also by dividing it between the categories of “Information Technology” and “Non-High-Technology” industries¹⁸. Further, I track this not only for the regions which contain the schools I investigate, but also for adjacent regions, and nationwide.

In addition to these measures of venture capital activity, I also include measures of population and personal income at the MSA level from the BEA. Population is simply the number of residents covered by the MSA, while personal income is total GDP at the MSA level, measured in thousands of dollars. For the latter, as with measures of venture capital activity, I transform all values to 2005 dollars. I then take the natural log of both variables, without adding any constants because both population and personal income are positive for all observations.

Finally, because general measures of population and economic activity may not correspond exactly to specific field of life-science innovation, and because fixed effects at the region- and year-levels may not capture all relevant forms of variation, I include the variable $\log(AcadRank_{rt})$, which captures the academic rank of schools within region r during year t . This measure is based on previously-described surveys of graduate program quality performed by the ACE and NRC. However, these surveys occurred approximately once per decade, with the first survey taking place in 1964 and the most recent in 2006. Further, schools are ranked on multiple departments in each survey. To address these issues, I run a set of school-specific regressions fitting a quadratic time-trend to the reported ranks across all surveys. I treat all observations equally, but take logs of the reported rankings in order to use a geometric rather than an arithmetic mean when aggregating departments with different ranks¹⁹. I use the fitted values from these school-specific regressions to measure the

¹⁸These classifications are based on the Venture Economics Industry Codes present in Thomson Financial databases.

¹⁹The use of the geometric mean aims to capture the fact that the difference between the first and tenth schools on the list is larger than the difference between the 21st and 30th. Alternative functional forms for

academic rank of school s in year t , and take the average of these fitted log-rank values (i.e. the geometric mean of the actual ranks) when aggregating this measure at the regional level.

3.4.3 Measures of Innovation

In the second stage of the analysis, I examine the impact of funding on innovation. In OLS specifications, I use the actual values of $\log(VC_{rt}^{LifeSci})$, as described above, to measure funding availability. In the IV specifications, I use the fitted values from the first-stage regressions. I also continue to use the measures of population, personal income, and school rank described above. In addition to these controls, I introduce a measure of researcher tenure, $\log(Tenure_{jt})$, which equals the natural log of the number of years between year t and the application year of the first patent on which researcher j is listed as an inventor²⁰. Further, I track the total number of inventors, and whether researcher j is listed as the first (and therefore most prominent) inventor on the patent. I also track the number of assignees listed on the patent, and take advantage of assignee classifications to determine whether a given patent is assigned to only academic or non-profit institutions, or whether the private sector is involved in the research. Finally, when analyzing observations at the patent level, I also include fixed-effect controls for technological fields according to the patent class groupings developed by the NBER Patent Data Project, specifically taking advantage of the measure of technology subclass.

In addition to the control variables above, I develop a range of measures of innovative outputs to serve as the dependent variables in my analysis. The first measure deals directly with the theoretical prediction along the extensive margin: transitions of researchers between academia and the private sector. I measure these transitions through the variable $Transition_{jt}$, which is defined as an indicator for whether any of the patents that inventor j applies for in year t include a private-sector assignee. For this measure to be meaningful,

combining ranks of multiple departments and interpolating for the years between surveys do not lead to any meaningful changes in the results.

²⁰I set tenure equal to one in the year that the first patent application is submitted to avoid undefined values when taking logs.

I restrict the sample to years in which the researcher applies for at least one patent of any kind, and focus on researchers who have applied for an academic patent in the past three years. These restrictions make sure that my results are not biased by researchers who are not producing any patents, or researchers who have left academia. The second dependent variable I construct is the $\log(Patents_{jt})$ measure, which tracks the number of patent applications submitted in year t which list researcher j as an inventor²¹. I also dis-aggregate this measure into academic and industry linked patent production in the same manner, tracking these outputs through the variables of $\log(Patents_{jt}^{Acad})$ and $\log(Patents_{jt}^{Ind})$, respectively.

In addition to measures of career trajectory and the quantity of innovation, I construct several measures of the nature of innovation. I track the citations to the patents in my sample using the variable $\log(Citations_{jt})$, which is calculated as the natural log of (one plus) the number of citations that accrue to the patents that researcher j applies for in year t . Because these are forward-looking measures, I designate fixed time horizons in order to create a valid metric for patents across a broad range of vintages. For my primary measure, I track all citations occurring within ten years of the patent's application. I also divide the citations I observe into short-term and long-term citations, with a range of zero to five years for the former and six to ten years for the latter. Finally, I measure the scope of innovation by tracking the number of distinct patent classes covered by the citations to any given patent. Specifically, I calculate the variable $\log(CitingClasses_{jt})$ as the log of the number of (distinct) patent classes covered by all citations accruing to the patents that researcher j applies for in year t . As with the number of citations, I impose a horizon of ten years in order to effectively compare patents of different vintages²².

In addition to the details above, I also document the definitions and sources of key variables in Table 1, and present summary statistics in Table 2. The following section proceeds to describe the main results of the empirical analysis.

²¹As this is a count variable, I add 1 to all observations before taking logs to avoid undefined values.

²²While I do include fixed effects at the annual level for all regressions, it is reasonable to assume that for any given patent, a citation within ten years of the original application is quite different from a citation occurring twenty years later. I therefore impose a ten-year horizon to keep the meaning of a citation relatively constant across the sample.

Table 3.1: Variables and Definitions

TABLE 1: VARIABLES & DEFINITIONS		
VARIABLE	DEFINITION	SOURCE
MSA-YEAR CHARACTERISTICS		
log(Life-Science Investment)	Natural log of the combined value of all VC investment in life-science ventures for a given MSA in given year	Thomson
Instr. log(Life-Science Investment)	Fitted values, based on the analysis in Table 4B, for the above measure of the log of life-science investment	Thomson
log(Life-Science IPOs)	Natural log of the combined value of all venture-backed IPOs in the life-science for a given MSA in given year	Thomson
log(Non-Life-Science IPOs)	Natural log of the combined value of all venture-backed IPOs outside the life-science for a given MSA in given year	Thomson
log(Population)	Natural log of total population for a given MSA in a given year	BEA
log(Income)	Natural log of income, or GDP, for a given MSA in a given year	BEA
log(School Rank)	Natural log of graduate program rankings, averaged across life science fields, according to surveys of academic reputation	NRC
INVENTOR-YEAR CHARACTERISTICS		
Tenure	Number of years from an inventor's first patent application, starting at one for the first application	NBER Patents
log(Tenure)	Natural log of an inventor's tenure in a given year	NBER Patents
Patents	Number of patents applied for by a given inventor in a given year.	NBER Patents
log(Patents)	Natural log of one plus the number of patent applications	NBER Patents
log(Academic Patents)	Natural log of one plus the number of patent applications containing only academic assignees	NBER Patents
log(Industry Patents)	Natural log of one plus the number of patent applications containing at least one private-sector assignee	NBER Patents
INVENTOR-YEAR CHARACTERISTICS CONDITIONAL ON AT LEAST ONE PATENT		
Career Transition Indicator	Indicator for whether or not an inventor applies for at least one industry patent in a given year, conditional on publishing an academic patent in the previous three years	NBER Patents
Academia Indicator	Indicator for whether or not a given inventor has submitted patent applications with only academic assignees throughout his patent history prior to a given year	NBER Patents
Citations	Number of citations received by all patents applied for by a given researcher in a given year, accruing within ten years of application	NBER Patents
log(Citations)	Natural log of the number of citations received by all patents applied for by a given researcher in a given year	NBER Patents
log(Short-Term Citations)	Natural log of the number of citations received by all patents, for years 0-5 following the application year	NBER Patents
log(Long-Term Citations)	Natural log of the number of citations received by all patents, for years 6-10 following the application year	NBER Patents
Citing Classes	Number of patent classes covered by the citations to all patents applied for by a given researcher in a given year, accruing within ten years of application	NBER Patents
log(Citing Classes)	Natural log of the number of patent classes to citations of all patents applied for by a given researcher in a given year	NBER Patents

Table 3.2: Summary Statistics

TABLE 2: SUMMARY STATISTICS					
VARIABLE	N	MEAN	STD. DEV.	MIN	MAX
MSA-YEAR CHARACTERISTICS					
log(Life-Science Investment)	770	2.44	2.68	-1.49	7.35
Instr. log(Life-Science Investment)	770	2.44	2.45	-4.62	7.39
log(Life-Science IPOs)	770	2.29	1.30	1.55	6.80
log(Non-Life-Science IPOs)	770	2.84	1.63	1.73	8.10
log(Population)	770	14.15	1.43	11.36	16.76
log(Income)	770	10.73	1.54	7.32	13.77
log(School Rank)	770	2.47	0.64	0.38	3.59
INVENTOR-YEAR CHARACTERISTICS					
Tenure	24115	4.93	4.43	1	26
log(Tenure)	24115	1.20	0.91	0	3.26
Annual Patents	24115	0.85	1.12	0	32
log(Patents)	24115	0.50	0.46	0	3.50
log(Academic Patents)	24115	0.40	0.43	0	3.43
log(Industry Patents)	24115	0.09	0.28	0	2.83
INVENTOR-YEAR CHARACTERISTICS CONDITIONAL ON AT LEAST ONE PATENT					
Career Transition Indicator	3891	0.22	0.41	0	1
Academia Indicator	8250	0.68	0.46	0	1
Citations	8250	17.26	36.25	0	976
log(Citations)	8250	2.13	1.22	0	6.88
log(Short-Term Citations)	8250	1.49	1.06	0	5.95
log(Long-Term Citations)	8250	1.61	1.18	0	6.39
Citing Classes	8250	3.88	3.90	0	48
log(Citing Classes)	8250	1.35	0.69	0	3.89

3.5 Results

The results of the empirical analysis are divided into two main lines of inquiry. I begin by addressing the first stage of the empirical analysis: the process of reinvestment in ven-

ture capital. Specifically, I explore reinvestment along the three dimensions of separation described in Section 3: lifecycle dynamics, inter-industry effects, and local reinvestment. Tables 3A and 3B explore lifecycle dynamics for same-industry and cross-industry reinvestment, respectively, while Tables 4A and 4B do the same for local effects. In addition, Table A1 in the appendix offers further robustness tests for the first-stage analysis. The second set of results focuses on the impact of funding on innovation, and presents both OLS estimates and instrumental-variables results based on fitted values from the first stage. Table 5 explores career transitions, Table 6 focuses on the quantity of patents, Tables 7 and A2 address patent quality through the number of citations, and Table 8 investigates patent scope through the patent classes of citations. The following sub-sections describe the results from each of these tables in detail.

3.5.1 First Stage: Reinvestment in Venture Capital

The results begin with Table 3A, which illustrates the reinvestment dynamics of venture capital within the life-science sector. The dependent variable is $\log(VC_{rt}^{LifeSci})$: the log of venture capital investment in the life sciences for a given MSA in a given year. The explanatory variables consist of lags and cumulative sums of venture-backed IPOs occurring in the same MSA. Specifically, I take the log of the combined value of all local IPOs over a given year or set of years. Specification 3A-1 includes each lag separately, from one to four years, as well as the contemporary measure. Shifting the lag structure to cumulative variables yields a very similar result in specification 3A-2: in the second column, the n th lagged measure includes all IPOs from years $t - 1$ to $t - n$, inclusive. Again, the results show that only the current year has a significant impact on current life-science investment. Because I employ a log-log framework, the economic significance of the results is easily interpreted: a 10% rise in local life science IPO activity leads to an approximately 1.2% increase in local life science investment. In both specifications, I also report the impact of academic rank and local GDP. While these coefficients are not significant, they do point in the expected direction: regions containing schools with small-number ranks attract higher levels of investment, as do more prosperous regions.

The results in Table 3A indicate that the current level of IPO activity is the primary

determinant of new investment in the same industry, indicating that there is little separation stemming from the mechanics of venture capital reinvestment. Instead, within the life sciences, both IPOs and investment seem to be driven by common shocks. Importantly, these estimates are based on a difference-in-differences approach, and persist even after including both calendar-year and MSA-level fixed effects.

Table 3.3: Reinvestment Dynamics

TABLE 3A: OWN-INDUSTRY REINVESTMENT DYNAMICS		
	3A-1 Dep. Var. = Log(Local Life-Science VC Investment) Year-by-Year	3A-2 Cumulative
Log(Local Life-Science IPOs)		
Contemporary IPOs	0.122** (0.044)	0.128** (0.045)
IPO Lag: 1 year	0.082** (0.037)	0.045 (0.043)
IPO Lag: 2 years	0.010 (0.038)	-0.022 (0.045)
IPO Lag: 3 years	0.051 (0.040)	-0.022 (0.073)
IPO Lag: 4 years	0.063 (0.047)	0.150 (0.112)
Log(Academic Rank)	-0.237 (0.354)	-0.258 (0.336)
Log(MSA Income)	2.709 (1.891)	2.603 (1.815)
Time Controls	FE	FE
MSA Controls	FE	FE
Level of Observation	MSA-Year	MSA-Year
N. of Observations	770	770
N. of MSAs	22	22

Standard errors clustered by Region in parentheses. Significance: * 10% ** 5% *** 1%

TABLE 3B: CROSS-INDUSTRY REINVESTMENT DYNAMICS			
	3B-1 Dep. Var. = Log(Local Life-Science VC Investment) Year-by-Year	3B-2 Cumulative	3B-3 Cumulative
Log(Local Non-Life-Science IPOs)			
Contemporary IPOs	0.079** (0.034)	0.090** (0.037)	0.074** (0.034)
IPO Lag: 1 year	0.020 (0.037)	-0.040 (0.044)	-0.048 (0.047)
IPO Lag: 2 years	0.071** (0.033)	0.069** (0.031)	0.068** (0.030)
IPO Lag: 3 years	0.021 (0.033)	0.049 (0.062)	0.050 (0.062)
IPO Lag: 4 years	0.033 (0.040)	-0.004 (0.102)	-0.007 (0.104)
Log(Local Life-Science IPOs)			0.116** (0.043)
Log(Academic Rank)	-0.293 (0.366)	-0.282 (0.347)	-0.261 (0.341)
Log(MSA Income)	2.922 (1.943)	2.956 (1.956)	2.740 (1.901)
Time Controls	FE	FE	FE
MSA Controls	FE	FE	FE
Level of Observation	MSA-Year	MSA-Year	MSA-Year
N. of Observations	770	770	770
N. of MSAs	22	22	22

Standard errors clustered by Region in parentheses. Significance: * 10% ** 5% *** 1%

To compare own-industry and cross-industry dynamics, Table 3B replicates the above

analysis while substituting IPOs *outside* the life sciences as the main explanatory variables. This approach allows for a direct comparison of the separation between funding availability and innovation along the dimension of industry. In specification 3B-1, I analyze the lag structure on a year-by-year basis as in Table 3A, seeking to predict the same dependent variable. While this specification also exhibits a strong current-year relationship between local IPOs and local investment, the cross-industry approach also indicates that there is a strong relationship at the two-year lag horizon. This lines up well with the mechanics of capital redeployment in the VC industry, falling in the middle of the one-to-three-year range described by Nanda and Rhodes-Kropf (2012). This pattern continues to hold in specification 3B-2, where I once again switch to a cumulative metric of lagged IPOs. With IPOs outside the life sciences, this variation is not subsumed in the contemporary IPO measure as in Table 3A. This result supports the interpretation that cross-industry reinvestment dynamics can offer a source of variation which is uncorrelated with investment opportunities. While the magnitude of the effect is smaller than that of contemporary, same-industry IPOs, it is nevertheless economically significant, leading to a 0.7% increase in investment in response to a 10% increase in local IPO activity.

As an additional test of whether cross-industry IPOs are conditionally uncorrelated with the prospects of new ventures, I include the contemporary life-science IPO measure that was found to be a strong predictor of life science investment. I report the results in specification 3B-3, and find that adding this explanatory variable has a negligible impact on the explanatory power of (two-year) lagged IPOs outside the life sciences. This lends credence to the argument that lagged cross-industry IPOs offer a source of potentially exogenous variation in funding availability for life-science investment.

In the next two tables, I investigate the complementary dimension of geography, in order to evaluate the degree to which local variation drives funding availability. I again divide my analysis between own-industry and cross-industry effects, and examine the former in Table 4A. This table seeks to explain local investment in life-science ventures by comparing the relative impact of local, regional, and national life-science (venture-backed) IPO levels. I continue to focus on local life-science investment, as captured by $\log(VC_{rt}^{LifeSci})$. In specification 4A-1, I use explanatory variables covering a progressively more distant set of life-science IPOs. Based on the results in Tables 3A and 3B, I attempt to isolate varia-

tion based on reinvestment by focusing on a cumulative measure of the past two years of local life-science IPOs. Incorporating this measure at a range of geographic levels, I find that national variation is the strongest driver of own-industry investment: nation-wide IPOs have a strong positive effect on investment. In specification 4A-2, and in all second-stage regressions, I include calendar-year fixed effects which subsume national-level variation in IPOs and other relevant factors. However, I can still compare the impact of own-MSA and adjacent-MSA IPOs, and find that past local IPOs do have a positive impact in subsequent local investment in the same industry. While the results in Table 3A indicated that this effect is more effectively captured by contemporary IPOs, this specification nevertheless shows that the relevant variation is local on a the relatively small scale of an MSA. This finding also matches well with the nature of venture capital investing, where monitoring and local information are important drivers of value.

Table 3.4: Local Effects

TABLE 4A: OWN-INDUSTRY LOCAL EFFECTS		
	4A-1	4A-2
	Dep. Var. = Log(Local Life Science VC Investment)	
Log(Life-Science IPOs, Past 2 Years)		
Same MSA	0.037 (0.049)	0.087* (0.046)
Adjacent MSAs	-0.024 (0.051)	0.003 (0.061)
Nationwide	0.162** (0.066)	
Log(Academic Rank)	-0.276 (0.362)	-0.301 (0.354)
Log(MSA Income)	4.923*** (1.525)	3.236 (1.913)
Time Controls	Polynomial	FE
MSA Controls	FE	FE
Level of Observation	MSA-Year	MSA-Year
N. of Observations	770	770
N. of MSAs	22	22

Standard errors clustered by Region (CSA) in parentheses. Significance: * 10% ** 5% *** 1%

TABLE 4B: CROSS-INDUSTRY LOCAL EFFECTS		
	4B-1	4B-2
	Dep. Var. = Log(Local Life Science VC Investment)	
Log(Non-Life-Science IPOs, Past 2 Years)		
Same MSA	0.075* (0.042)	0.085** (0.039)
Adjacent MSAs	0.030 (0.080)	0.018 (0.075)
Nationwide	0.102 (0.085)	
Log(Academic Rank)	-0.255 (0.337)	-0.287 (0.338)
Log(MSA Income)	4.106** (1.463)	3.224 (1.971)
Time Controls	Polynomial	FE
MSA Controls	FE	FE
Level of Observation	MSA-Year	MSA-Year
N. of Observations	770	770
N. of MSAs	22	22

Standard errors clustered by Region in parentheses. Significance: * 10% ** 5% *** 1%

In Table 4B, I continue the analysis of local effects by applying the same framework to the cross-industry impact of IPOs on subsequent investment. In specification 4B-1, I again

compare the effects of local, regional, and national IPO activity over the previous two years on current investment in life science ventures. Unlike the own-industry analysis, I find that the local impact is closest to statistical significance, and that the local and national effects are relatively close in economic significance. With the inclusion of calendar-year fixed effects in specification 4B-2, the impact of local cross-industry IPOs becomes stronger in both statistical and economic significance. This result indicates that variation at the metropolitan level is the most effective predictor of subsequent investment, as IPOs in adjacent MSAs have a near-zero contribution. The controls of academic rank and local MSA income remain insignificant as in Table 3B, but do point in the expected direction. This final specification combines all three dimensions of separation: it takes advantage of venture capital cycles by targeting lags that match reinvestment horizons, incorporates cross-industry effects to avoid alternative channels of influence, and targets geographically-concentrated effects which correspond to the local nature of the VC industry. By combining these dimensions, this specification presents evidence for a measure of funding availability that is uncorrelated with investment opportunities in the life sciences. I therefore use this regression to construct my instrumented measure of local VC investment, and bring the fitted values to the second stage to evaluate the impact of funding availability on innovation.

In addition to the results above, I report additional supporting evidence in Table A1 of the Appendix. Using similar methodology, I show that my results continue to hold if I restrict the analysis to regions where venture capital is not a dominant industry, i.e. regions outside of New York, Boston, and the San Francisco Bay Area. I also repeat the analysis using numbers of IPOs rather than IPO valuations. For an additional level of separation, I also calculate a “fitted IPOs” measure by using historical IPO shares, constructed by looking at the fraction of national IPOs which occur in a given MSA based on lags from six to ten years. I then multiply this historical share with contemporary national IPOs to obtain a “fitted” measure, and find that it is also an effective predictor of subsequent cross-industry investment. Finally, I separate IPOs into the two primary venture categories outside the life sciences: Information Technology and Non-High-Technology fields. While both industries predict a positive response in subsequent cross-industry investment, IT seems to be the stronger of the two. Having established this range of robustness tests, I now proceed to use the fitted values from specification 4B-2 as instruments for funding availability as I

investigate its impact on innovation in the life sciences.

3.5.2 Second Stage: Funding and Innovation

This section describes the second stage of the empirical analysis, where I investigate the impact of funding availability on innovative output. I focus on four specific measures: career transitions, and the quantity, quality, and scope of innovation. The results are presented in Tables 5 through 8, and I describe them in detail below.

Career Transitions

The first measure of innovation that I investigate is that of career transitions. As described in Section 4, the dependent variable $CareerTransitions_{jt}$ tracks whether researcher j has a collaboration with industry in year t . Specifically, I measure whether the researcher applies for a patent which includes at least one private-sector assignee. In order for this measure to be effective at capturing transitions, I restrict the sample to years in which the researcher applies for at least one patent of any kind, and focus on researchers who have applied for an academic patent in the past three years. These restrictions make sure that my results are not biased by researchers who are not producing any patents, or researchers who have left academia. The explanatory variables of focus are measures of local VC investment in the life sciences, either by using $\log(VC_{rt}^{LifeSci})$ directly in OLS specifications or using the fitted values resulting from the first stage to instrument for the same variable. I also include a full set of fixed effects at the calendar-year, school, and inventor level, and controls based on academic rank, MSA population and income, and inventor tenure. In specification 5-1, I find a positive impact of funding on career transitions under OLS, with the effect concentrated in the current year. The effect is statistically significant, and the transition-propensity framework implies that a 10% increase in investment leads to a 12 basis point increase in the likelihood of transition. The baseline transition probability in the restricted sample is approximately 20%, indicating that the OLS estimate is of modest impact. However, this estimate is likely to be biased: if the level of investment is responding to an increase in the value of innovation, this is likely to correlate with increases in the attractiveness of both academia and the private sector for researchers. This

will mitigate the differential effect between the two, and reduce the estimated impact on career transitions.

To address the shortcomings of the OLS approach, specification 5-2 incorporates instruments for local investment levels into the above framework. The point-estimates for the impact of both contemporary and lagged investment increases substantially. While the contemporary measure remains much larger in economic significance, statistical significance shifts to the lagged measure of investment. This supports the interpretation that the instrumental approach offers a measure of funding availability which is uncorrelated with investment opportunities - the lagged relationship is less likely to be driven by an omitted variable simultaneously influencing both funding and investment, and the increase in effect relative to specification 5-1 matches expectations based on the shortcomings of the OLS approach. This suggests that a causal interpretation is possible for the IV estimates, which indicate that a 10% increase in local investment leads to a 20-basis-point rise in the likelihood of transition, or a 1% rise relative to the baseline likelihood of 20%.

Table 3.5: Career Transitions

TABLE 5: CAREER TRANSITIONS		
Specification: Dep. Var.	5-1 OLS	5-2 IV
	Transition Indicator	
Log(Local VC Investment)		
Contemporary Investment	0.012** (0.006)	
Lag: 1 year	0.005 (0.007)	
Instrumented Log(VC Investment)		
Contemporary Investment		0.091 (0.104)
Lag: 1 year		0.020** (0.010)
Log(Inventor Tenure)	0.060 (0.038)	0.059 (0.038)
Time Controls	FE	FE
School Controls	FE	FE
Inventor Controls	FE	FE
Level of Observation	Inventor-Year	Inventor-Year
N. of Observations	3891	3891
N. of Inventors	1007	1007

Standard errors clustered by Inventor in parentheses. Significance: * 10% ** 5% *** 1%

Quantity of Innovation

In Table 6, I explore the impact of funding availability on the quantity of innovative output. This analysis addresses two important theoretical predictions: first, that stronger incentives and greater access to funding will increase innovative output, and second, that funding has a differentially strong impact on patenting relative to other activities pursued by innovators in the life sciences.²³ The dependent variables in this table are $\log(Patents_{jt})$ and $\log(Patents_{jt}^{Acad})$, representing the log of one plus the number of patents (or academic patents) applied for by inventor j in year t . The main explanatory variables are the same as in Table 5: local life-science VC investment and its instrumented version. I focus on contemporary measures here, as lagged measures do not provide significant explanatory power. I again include a full set of fixed effects and a range of controls based on school, MSA, and inventor characteristics. The sample in these regressions is unrestricted: because an observation of no patents is meaningful, I include all years between the first and last observation of a patent application for each inventor.

Specifications 6-1 and 6-2 present the OLS results, which indicate either no effect of funding or a very small negative impact. This non-result seems to favor the interpretation that researchers have significant alternatives to patenting which also increase in value when investment opportunities improve. This interpretation is supported by the positive results under an IV approach in specifications 6-3 and 6-4. I find significant effects for both all patents and for purely academic patents, with a 10% increase in investment leading to a 0.8% increase in all patenting and a 1.1% increase in academic patents. Omitted here is the result for private-sector patents, which do not exhibit any significant response to local investment. These findings lend support to the prediction that increased funding availability differentially promotes patent-focused innovation relative to the alternative activities in which inventors might choose to engage. This effect is concentrated in academia, indicating that private-sector innovation is relatively less sensitive to local funding availability, possibly due to access to a broader range of funding options. Overall, the results in Ta-

²³In a sample with strong academic inclinations, more theoretically-inclined research, an emphasis on publication, or effort expended on teaching and conference organization are likely to be prime alternatives to patent-focused innovation.

ble 6 highlight the finding that under the freedom offered in academia, inventors respond strongly to the incentives of increased funding availability by shifting their focus toward patent-linked innovation.

Table 3.6: Patent Quantity

TABLE 6: PATENT QUANTITY				
Dep. Var.	6-1	6-2	6-3	6-4
	OLS Log(Patents)	OLS Log(Acad. Patents)	IV Log(Patents)	IV Log(Acad. Patents)
Log(Local VC Investment)	-0.003 (0.004)	-0.007* (0.004)		
Instrumented Log(VC Investment)			0.083* (0.046)	0.111*** (0.041)
Log(Academic Rank)	-0.068 (0.073)	0.052 (0.067)	-0.035 (0.074)	0.100 (0.068)
Log(Inventor Tenure)	-0.324*** (0.011)	-0.256*** (0.011)	-0.324*** (0.011)	-0.256*** (0.011)
Inventor Controls	FE	FE	FE	FE
Time Controls	FE	FE	FE	FE
School Controls	FE	FE	FE	FE
Level of Observation	Inventor-Year	Inventor-Year	Inventor-Year	Inventor-Year
N. of Observations	24115	24115	24115	24115
N. of Inventors	6587	6587	6587	6587

Standard errors clustered by Inventor in parentheses. Significance: * 10% ** 5% *** 1%

Value of Innovation

Having presented evidence for a rise in the quantity of innovation in response to funding availability, I now turn to the impact of funding on the nature of innovation. The first dimension I explore is that of the “value of innovation” which I measure by looking at the number of citations a given patent receives from subsequent innovations. As described in Section 4, this approach has been widely used in the study of innovation, and the details of using this metric are discussed by Henderson et al (1998). In Table 7, I present results based on citations occurring in the first ten years following the patent’s application. In Appendix Table A2, I further divide these results between short term (0-5 year) and long-term (6-10 year) citation totals. In both tables, I use a log-based metric: Table 7 focuses on $\log(Citations_{jt})$, which equals the log of one plus the number of citations received by

inventor j for patents applied for in year t . For all specifications, I control for whether or not the inventor is in academia based on the assignees of prior patents. This means that at least one patent is necessary for the identification, so I restrict the sample to the years after a given inventor's first patent. Further, in order to highlight a different dimension from that of patent quantity, I exclude all years in which a given inventor does not produce any patents, as these years would mechanically lead to a lack of subsequent citations.

For analyzing the nature of innovation, I focus on lagged measures of investment covering the two years preceding a given application year. Thus, I take the log of *total* life-science investment over the past two years for both direct and instrumented measures. Further, to explore the possibility of a differential effect between academia and the private sector, I interact my lagged measure of investment with the academia indicator described above. As in previous regressions, I include a full set of fixed effects and controls in all specifications. I begin with specifications 7-1 and 7-2, which present the OLS results. I find a consistent positive effect of being an academic on citation rates in both specifications, in line with the theoretical prediction that academia focuses on earlier-stage research. In evaluating the impact of funding availability, I find that the interaction between funding and academia has a significant negative effect: it is associated with academic research which is less valuable as a foundation for future inventions. Specifications 7-3 and 7-4 show that this pattern continues to hold under an instrumental-variables approach, with the interaction term between academia and local life-science investment having a significant negative impact on citation rates. In terms of economic magnitude, the IV results virtually identical to those from the OLS specification, with the coefficient on the interaction moving from -0.061 under OLS to -0.059 under IV. This is not surprising: while innovation opportunities may play an important part in the behavior of the original inventor, such variation is unlikely to have a strong impact on citations received over the decade following a patent's application.

Table 3.7: Patent Value

TABLE 7: PATENT CITATIONS				
Specification: Dep. Var.	7-1 OLS	7-2 OLS	7-3 IV	7-4 IV
	Log(Citations)			
Academia Indicator	0.107* (0.061)	0.119* (0.061)	0.108* (0.061)	0.117* (0.061)
Past 2 years: Log(Local VC Investment)	-0.005 (0.019)	-0.011 (0.020)		
Academia X Log(VC Investment)		-0.061** (0.024)		
Instrumented Log(VC Investment)			-0.002 (0.058)	-0.006 (0.059)
Academia X Instrumented Log(VC)				-0.059** (0.029)
Inventor Controls	FE	FE	FE	FE
Time Controls	FE	FE	FE	FE
School Controls	FE	FE	FE	FE
Level of Observation	Inventor-Year	Inventor-Year	Inventor-Year	Inventor-Year
N. of Observations	8250	8250	8250	8250
N. of Inventors	3208	3208	3208	3208

Standard errors clustered by Inventor in parentheses. Significance: * 10% ** 5% *** 1%

In Appendix Table A2, I break down citation rates between short-term (0-5 year) and long-term (6-10 year) citations. The results largely mirror those in Table 7, indicating that academic innovations receive greater numbers of citations but that increased funding availability leads to a decrease in citation rates for academic researchers relative to their private-sector counterparts. The key findings from this table are based on the comparison of the short-term and long-term results. The higher baseline citation rate for academics is present most strongly in short-term citations, while the funding-driven decrease in the citations to academic research is strongest in long-term citations.

Innovation Scope

In the final set of results, I explore the impact of funding availability on the scope of innovation. This measure is closely-tied to several theoretical predictions. First, in a multi-stage innovation setting, the earliest stages are predicted to occur in academia, and these stages are also predicted to have the broadest potential applications. This implies that academic innovations will be broader in scope relative to those in the private sector.

Second, funding availability may well have a differential impact between academia and the private sector, pushing academics toward later-stage projects which are closer to the point of commercialization while having relatively little impact on project choice in the private sector. To measure the scope of an inventor's innovations, I focus on the number of patent classes represented in the citations to the inventor's patents. Specifically, I use the variable $\log(CitingClasses_{jt})$, which is calculated as the log of the number of patent classes covered by the citations to *all* patents applied for by inventor j in year t . As in the analysis of citation rates, I measure the number of classes based on a ten-year horizon from the application year. Note that because the number of citing patent classes has a strict upper bound due to the finite number of categories used by the USPTO, there is no need to take the log of this measure. For these regressions, I again use a measure of local VC investment based on the total over the past two years, and include an academia indicator and an interaction between it and local investment.

I begin the analysis with specifications 8-1 and 8-2, which report the results from the OLS specifications. The point estimates reveal a pattern of results that is similar to those for citation rates: academia has a positive baseline effect on the scope of innovation, while the interaction between funding availability and academia is negative. While the estimates on the academia indicator are noisy, the impact of the interaction is significant, with a coefficient of -0.030. Repeating the analysis while instrumenting for funding availability in specifications 8-3 and 8-4 reveals the same pattern of point estimates, with a slight increase in the magnitude of the coefficient to -0.016. As in past regressions, the log-log framework allows for straightforward interpretation, indicating modest decreases of 0.30% and 0.38% under OLS and IV, respectively, in response to a 10% increase in local investment. As in Table 7, the magnitude of the interaction remains similar when moving from the OLS to the IV specification. This indicates that endogeneity is not a significant driver of variation along the dimension of innovative scope; this is likely due to the fact that citations are not location-specific, and accrue to a patent over a decade-long horizon. Overall, the main finding in this section is that for academic researchers, greater funding availability leads to narrower innovations, confirming the theoretical prediction that academics will shift to later-stage projects during high-funding states.

Table 3.8: Citation-Linked Patent Classes

TABLE 8: CITATION-LINKED PATENT CLASSES				
Specification: Dep. Var.	8-1 OLS	8-2 OLS	8-3 IV	8-4 IV
	Log(Number of Citing Patent Classes)			
Academia Indicator	0.035 (0.033)	0.041 (0.033)	0.035 (0.033)	0.041 (0.033)
Past 2 years: Log(Local VC Investment)	0.005 (0.011)	0.002 (0.011)		
Academia X Log(VC Investment)		-0.030** (0.013)		
Instrumented Log(VC Investment)			-0.005 (0.029)	-0.008 (0.029)
Academia X Instrumented Log(VC)				-0.038** (0.015)
Inventor Controls	FE	FE	FE	FE
Time Controls	FE	FE	FE	FE
School Controls	FE	FE	FE	FE
Level of Observation	Inventor-Year	Inventor-Year	Inventor-Year	Inventor-Year
N. of Observations	8250	8250	8250	8250
N. of Inventors	3208	3208	3208	3208

Standard errors clustered by Inventor in parentheses. Significance: * 10% ** 5% *** 1%

3.6 Conclusion

In this paper I argued that fluctuations in funding availability have a significant impact on not only the quantity but also the nature of innovation. I develop a theory of credit constraints in multi-stage innovation, leading to the prediction that greater funding pushes inventors to transition to the private sector and to engage in a greater number of shorter-horizon, narrower-scope projects. I test these hypotheses using a panel of life-science researchers linked to top US universities, tracking their patent output from 1975 through 1999. To address the challenges of omitted variables and reverse causality in the relationship between funding and innovation, I adopt an instrumental-variables approach based on venture-backed IPOs outside the life sciences. In the first stage, I find that local variation is an important driver of venture capital investments, and that cross-industry effects persist even after controlling for own-industry conditions. Using this source of conditionally uncorrelated variation in funding availability, I examine the impact of funding availabil-

ity on early-stage innovation. In line with theoretical predictions and previous studies, I find that greater funding availability leads to an increase in transitions from academia to the private sector, and an increase in the quantity of innovation. However, I also present evidence for two novel dimensions of impact: I find that greater funding availability leads to a decrease in citation rates, citation horizons, and the scope of applicability for innovative output. These findings imply a tradeoff between focused, high-output development of existing technologies under high funding levels, and earlier-stage exploration of new technologies when funding is lower.

The analysis developed in this paper could be extended in several interesting directions. While previous research has often focused on the impact of policy changes on the quantity of innovation, the impact on the nature of innovation remains relatively unexplored. Important examples of such a policy changes include the Employee Retirement Income Security Act of 1979, and the JOBS Act of 2012. Such policy changes are often promoted as having the benefit of increasing the availability of funding for new businesses, and encouraging greater levels of innovation and job creation. The findings of this paper suggest that the impact of such policies is more complex than a simple increase in the quantity of innovation. Importantly, an attempt to promote private-sector funding for early-stage research may prove counter-productive, as the reduction of exploration may severely limit both the scope and total value of follow-on research stemming from the early-stage innovations. Further, the impact of funding is likely to have important interactions with intellectual property restrictions and the organizational structures used in managing innovation in both corporate and academic contexts. This suggests the need for a systematic analysis of the forces and trade-offs at work in an economic environment where inventors compete for both funding and recognition while choosing among a range of potential research paths which set the stage for future innovations.

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Appendices

Appendix to Chapter 1

We broadly define openness as any event or device that increases a researcher's ability to access the ideas or materials of other researchers. Alternatively, it allows a researcher to provide access to her own ideas and share them as she sees fit. There are two main approaches to analyze the effects of openness on basic research and innovation. The appropriability approach stresses the idea that openness should favor more applied research, generally at the expense of more basic research, as it reduces the extent to which upstream researchers can appropriate the returns from their own research.²⁴ The control approach²⁵ predicts instead that increased openness should foster basic research and the creation of new lines, in particular by reducing researchers' cost of accessing other researchers' ideas, thereby making it more likely that the alternative strategies pursued by researchers with high levels of freedom will actually lead to new lines. In particular, this latter approach emphasizes the complementarity between openness and freedom and the resulting effect of openness on the diversity of subsequent research lines.

The appropriability view

Consider a two-stage research line. Each stage requires one researcher. Success at each stage, occurs with probability p , and moves research to the next stage. As long as we focus

²⁴Although openness makes it also more likely that stage- $i + 1$ researchers will know about, and therefore build upon, the ideas of stage- i researchers, which in turn should increase the ex ante incentive to undertake stage i .

²⁵See Aghion-Dewatripont-Stein (2009).

on a single research line, a first effect of openness is that it increases the extent to which stage 2 can extract rents from stage 1. Thus, if V denotes the ex post value of the line (e.g. the price at which the research can be commercialized), then the value Π_2 of the line as of stage 2, is equal to

$$\Pi_2 = pV + \psi - w,$$

where ψ is the additional rent openness gives stage 2 at the expense of stage 1, and w is the wage paid to a researcher (we take it as given for simplicity). The stage-1 value of the line can then be expressed as:

$$\Pi_1 = p(pV - \psi - w) - w = p^2V - p\psi - (1 + p)w.$$

Thus, trivially, increasing ψ fosters stage-2 research at the expense of stage 1 research since it raises Π_2 and reduces Π_1 . In this framework, openness is expected to reduce the incentives to create new lines (as it increases the probability that Π_1 will be less than the fixed cost K of creating a new line). Meanwhile, openness should foster increased downstream research thanks to higher appropriability.²⁶ Finally, note that the appropriability approach has nothing to say on the relationship between openness and the diversity of research lines.

The control view and the effect of openness on diversity

In this subsection we introduce the notion of academic freedom, and then analyze the interplay between freedom and openness. We argue that to the extent that early research

²⁶One can extend the analysis by assuming that openness has an additional effect, namely to increase the possibility for the stage-1 researcher to transmit her research to stage 2 researcher(s). Indeed, once success has been obtained in stage 1, it may not be immediate to identify a researcher who will be able to carry the project forward into stage 2. This may require a ‘successful match’, whose probability will naturally rise with openness. Specifically, we call the probability of such a match A and we assume it depends positively on ψ . This means the stage-1 value of the line becomes:

$$\Pi_1 = pA(\psi)(pV - \psi - w) - w = A(\psi)(p^2V - p\psi) - (1 + p)w.$$

In turn, this implies:

$$\frac{d\Pi_1}{d\psi} = A'(\psi)(p^2V - p\psi) - pA(\psi),$$

which can be positive in particular if the effect of openness on the quality of matching is high (i.e. if $A'(\psi)$ is high). If we assume that research is socially optimal (i.e. if $p(pV - w) - w > 0$), then a sufficient condition for openness to be efficiency-enhancing is that $\frac{d\Pi_1}{d\psi} > 0$.

stages are optimally managed under academic freedom, openness in early stages of research should foster the creation of new research lines.

Introducing academic freedom

We maintain the assumption that research proceeds along multi-stage lines, with each line starting with an initial idea I_0 , and eventually generating a marketable product with value V after $k \geq 2$ successful stages. As before, we assume that it is sufficient to hire one researcher per stage, and that this researcher obtains a probability of success equal to $p < 1$ at any stage if he follows the success-maximizing (“practical”) research strategy at that stage. But now we also assume that, instead of the practical strategy, a researcher is free to follow an “alternative” strategy. If we assume that the scientist has a zero individual probability of success following this approach, then this alternative strategy amounts to the scientist working on an activity that he enjoys more but that does not pay off in monetary terms. However, as we describe at the end of this section, we can interpret this alternative strategy as the case in which the scientist works on an activity that may help initiate new lines but does not generate progress on the initial line.

There is an infinite supply of researchers at each stage, each of whom has an outside option w . After being hired at stage j , the scientist is exposed to idea I_{j-1} , and then learns whether he would prefer following the practical strategy or the alternative strategy. If he is able to undertake his favored strategy, he suffers no disutility from working. If, however, the scientist has to undertake the strategy that he likes less, he suffers disutility of z . The ex ante probability that a scientist prefers to follow the practical strategy is given by α . Assume further that the choice of the practical vs. alternative strategy is ex ante non-contractible.²⁷

Academic research (or freedom) differs from private-sector research in that it leaves control rights over the research strategy in the hands of the researcher. Thus if a research line is pursued in academia, the researcher is paid wage w and always works on his preferred strategy. This implies that with probability α , the scientist works on the practical strategy, and with probability $(1 - \alpha)$, he works on the alternative strategy. Thus the ex

²⁷In other words, one cannot write a contract that promises a scientist a bonus for following the practical strategy, because the nature of what kind of work that strategy entails cannot be adequately described ahead of time.

ante probability of advancing to the next stage is given by αp . Now consider a researcher employed by the private sector. Whether the researcher prefers the practical or the alternative strategy, becomes evident once the researcher has been hired by the firm and has been given access to the idea by the firm owner. Yet *ex post*, the firm owner has the authority to force the scientist to work on the practical strategy. Anticipating this, the researcher will demand a wage of $w_p = w + (1 - \alpha)z$ in order to work in the private sector. The $(1 - \alpha)z$ markup over the academic wage represents compensation for loss of creative freedom—the fact that scientists now always have to adopt the practical strategy, whether this turns out to coincide with their preferences or not.

Complementarity between openness and freedom: diversification effects

A main finding in ADS is that academic freedom tends to dominate private sector focus at earlier stages on a research line. To see this, take a research line involving 2 stages, and suppose that the first stage has been successful, so that we are now at stage 2, with one more stage to be completed in order to generate a payoff of V . If this last stage of research is done in the private sector, the expected payoff is equal to $E(\pi_2^p) = pV - w_p$. If instead the last stage is done in academia, the expected payoff is equal to $E(\pi_2^a) = \alpha pV - w$. This means that private sector research will yield a higher payoff than free (academic) research and only if $(1 - \alpha)pV > (w_p - w)$, or equivalently $pV > z$.

Now, let Π_2 denote the maximum of $E(\pi_2^p)$ and $E(\pi_2^a)$. Moving back to stage 1, we now compare between $E(\pi_1^p) = p\Pi_2 - w_p$ and $E(\pi_1^a) = \alpha p\Pi_2 - w$. Private sector research will yield a higher payoff than free (academic) research at stage 1 if and only if $p\Pi_2 > z$.

Since $\Pi_2 < V$, it follows that private sector research is value-maximizing at stage 1, it is also value-maximizing at stage 2. In particular it cannot be value maximizing to have academic freedom operate at later stages than private sector research. The key result is therefore that academic freedom will be the optimal governance structure at earlier stages and private sector research will be optimal at later stages. The intuition is that while academia's wage cost advantage stays constant over research stages, its lower probability of success becomes more problematic as one approaches the final value V .

This result can be generalized to lines of any length k : if Π_i denotes the NPV of the

line of length k as of stage i , we have:

$$\Pi_i = \max\{E(\pi_i^p) = p\Pi_{i+1} - w_p, E(\pi_i^a) = \alpha p\Pi_{i+1} - w\} < \Pi_{i+1}.$$

This monotonicity property, together with the fact that research should be pursued under academic freedom if and only if $p\Pi_{i+1} > z$, yields the desired result.²⁸

That more openness should foster the creation of new lines, follows from the fact that openness favors the cross-fertilization of ideas within stages. More formally, consider two parallel research lines, 1 and 2, each of which operates as described above. Namely, with ex ante probability α the researcher initially allocated to the current stage of either of these two lines, prefers to pursue the practical strategy for that line whereas with probability $(1 - \alpha)$ he prefers not to pursue this practical strategy. Now openness implies that the scientist on line 1 can learn about project 2 and vice-versa, and that consequently with positive probability φ , thanks to academic freedom and the resulting horizontal interaction, she may choose to work on the practical strategy for project 2 if nobody else does. A greater degree of openness implies a higher value of φ .

Openness also increases the net present value of a research line operated under academic freedom in a given stage i , from:

$$\alpha p\Pi_i - w$$

to:

$$[\alpha + (1 - \alpha)\varphi]p\Pi_i - w.$$

Thus openness increases the social value of operating any stage (particularly earlier stages) under academic freedom.

²⁸Note that the model so far provides a rationale for free (academic) research even in the extreme case where the alternative strategy has no value beyond saving the researcher the disutility of pursuing the practical strategy. In reality however there is value in experimenting with ideas that may lead to an entirely new research lines, consistently with the idea that scientific discoveries do not follow a purely “linear” model. This does not alter the relative optimality of academia (vs. private research) in earlier (vs. later) stages of research. It does, however, raise the desirability of freedom in general (and academia as the institutional regime that supports such freedom), if we make the realistic assumption that pursuing the alternative strategy confers a higher probability of generating entirely new research lines than pursuing the practical strategy (note that, realistically, the probability of such an event, possibly the result of an “accidental” discovery, is nonzero for both strategies). See ADS for details.

Appendix to Chapter 2

Updating rule for the *terminate* policy

Given the market's conjecture that the firm has adopted the *efficient* policy, $\hat{D} = 0$, the conditional distribution of η given that the project failed is given by Bayes' Rule:

$$\begin{aligned}
 f(\eta|\text{Proj. Fails}) &= \frac{f(\eta) \Pr(\text{Proj. Fails}|\eta)}{\int_{\hat{\eta}} f(\hat{\eta}) \Pr(\text{Proj. Fails}|\hat{\eta}) d\hat{\eta}} \\
 &= \frac{f(\eta) [\pi(1 - \eta(1 - \delta)) + (1 - \pi)(1 - \eta)]}{\pi(1 - \mu(1 - \delta)) + (1 - \pi)(1 - \mu)} \\
 &= \frac{f(\eta) [1 - \eta(1 - \pi\delta)]}{1 - \mu(1 - \pi\delta)}
 \end{aligned}$$

Therefore, we can calculate the conditional expectation of η as:

$$\begin{aligned}
 E_{mkt}[\eta|\text{Proj. Fails}] &= \int_{\eta} \eta f(\eta|\text{Proj. Fails}) d\eta \\
 &= \frac{1}{1 - \mu(1 - \pi\delta)} \int_{\eta} \eta [1 - \eta(1 - \pi\delta)] f(\eta) d\eta \\
 &= \frac{\mu - E[\eta^2](1 - \pi\delta)}{1 - \mu(1 - \pi\delta)} \\
 &= \frac{\mu - (\mu^2 + \sigma_{\eta}^2)(1 - \pi\delta)}{1 - \mu(1 - \pi\delta)} \\
 &= \mu - \frac{\sigma_{\eta}^2(1 - \pi\delta)}{1 - \mu(1 - \pi\delta)}
 \end{aligned}$$

Similarly, the conditional distribution of η given that the project succeeded is given by the Bayes Rule:

$$\begin{aligned}
 f(\eta|\text{Proj. Succeeds}) &= \frac{f(\eta) \Pr(\text{Proj. Succeeds}|\eta)}{\int_{\hat{\eta}} f(\hat{\eta}) \Pr(\text{Proj. Succeeds}|\hat{\eta}) d\hat{\eta}} \\
 &= \frac{f(\eta) [\pi\eta(1 - \delta) + (1 - \pi)\eta]}{\pi\mu(1 - \delta) + (1 - \pi)\mu} \\
 &= \frac{f(\eta) [\eta(1 - \pi\delta)]}{\mu(1 - \pi\delta)} \\
 &= f(\eta) \frac{\eta}{\mu}
 \end{aligned}$$

Therefore, conditional expectation is

$$\begin{aligned}
 E_{mkt} [\eta | \text{Proj. Succeeds}] &= \int_{\eta} \eta \cdot f(\eta | \text{Proj. Succeeds}) d\eta \\
 &= \int_{\eta} \frac{\eta^2}{\mu} f(\eta) d\eta \\
 &= \frac{E[\eta^2]}{\mu} \\
 &= \frac{\sigma_{\eta}^2 + \mu^2}{\mu} \\
 &= \mu + \frac{\sigma_{\eta}^2}{\mu}
 \end{aligned}$$

Updating rule for the *delay* policy

Given the market's conjecture that the firm has adopted the defensive policy, $\hat{D} = 1$, the outcome of observing layoffs would be off the equilibrium path and Bayes' Rule would not apply. However, as specified in the main text, introducing trembles to the model allows for a positive probability of observing layoffs. Further, because the manager only has a decision point when the project fails, the observation of layoffs in this setting is equivalent to observing project failure. Thus, the application of Bayes' Rule is identical to the case of the *terminate* policy:

$$\begin{aligned}
 f(\eta | \text{Proj. Fails}) &= \frac{f(\eta) \Pr(\text{Proj. Fails} | \eta)}{\int_{\hat{\eta}} f(\hat{\eta}) \Pr(\text{Proj. Fails} | \hat{\eta}) d\hat{\eta}} \\
 &= \frac{f(\eta) [1 - \eta(1 - \pi\delta)]}{1 - \mu(1 - \pi\delta)}
 \end{aligned}$$

Again, we calculate the conditional expectation of η as:

$$\begin{aligned}
 E_{mkt} [\eta | \text{Proj. Fails}] &= \int_{\eta} \eta f(\eta | \text{Proj. Fails}) d\eta \\
 &= \mu - \frac{\sigma_{\eta}^2(1 - \pi\delta)}{1 - \mu(1 - \pi\delta)}
 \end{aligned}$$

For the case of no layoffs, because the market expects firms to never announce a layoff when $\hat{D} = 1$, the observation of no layoffs should not lead to any updating. This is exactly what we find:

$$\begin{aligned} f(\eta | \text{No Layoffs}, \hat{D} = 1) &= \frac{f(\eta) \Pr(\text{No Layoffs} | \eta)}{\int_{\hat{\eta}} f(\hat{\eta}) \Pr(\text{No Layoffs} | \hat{\eta}) d\hat{\eta}} \\ &= f(\eta) \end{aligned}$$

By inspection, the posterior distribution is identical to the prior, so the market's expectation of talent remains at μ .

Updating rule for a mixed-strategy policy

For the case where the market observes layoffs, the signal is once again equivalent to observing project failure. Thus, the calculation proceeds as in the previous sections, and yields the posterior expectation:

$$\begin{aligned} E_{mkt}[\eta | \text{Proj. Fails}] &= \int_{\eta} \eta f(\eta | \text{Proj. Fails}) d\eta \\ &= \mu - \frac{\sigma_{\eta}^2(1 - \pi\delta)}{1 - \mu(1 - \pi\delta)} \end{aligned}$$

For the case of no layoffs, the market expects firms to announce “no layoffs” with probability \hat{D} conditional on project failure, and with probability 1 conditional on project success. The updating rule proceeds as follows:

$$\begin{aligned} f(\eta | \text{No Layoffs}, \hat{D}) &= \frac{f(\eta) \Pr(\text{No Layoffs} | \eta, \hat{D})}{\int_{\hat{\eta}} f(\hat{\eta}) \Pr(\text{No Layoffs} | \hat{\eta}, \hat{D}) d\hat{\eta}} \\ &= \frac{f(\eta) [\eta(1 - \pi\delta) + \hat{D}(1 - \eta(1 - \pi\delta))]}{\mu(1 - \pi\delta) + \hat{D}(1 - \mu(1 - \pi\delta))} \end{aligned}$$

Therefore, the conditional expectation is

$$\begin{aligned}
 E_{mkt} [\eta | \text{No Layoffs}, \hat{D}] &= \int_{\eta} \eta f(\eta | \text{No Layoffs}, \hat{D}) d\eta \\
 &= \frac{E[\eta^2](1 - \pi\delta) + \hat{D}(\mu - E[\eta^2](1 - \pi\delta))}{\mu(1 - \pi\delta) + \hat{D}(1 - \mu(1 - \pi\delta))} \\
 &= \frac{(\mu^2 + \sigma_{\eta}^2)(1 - \pi\delta) + \hat{D}(\mu - (\mu^2 + \sigma_{\eta}^2)(1 - \pi\delta))}{\mu(1 - \pi\delta) + \hat{D}(1 - \mu(1 - \pi\delta))} \\
 &= \mu + \frac{\sigma_{\eta}^2(1 - \pi\delta) + \hat{D}(-\sigma_{\eta}^2)(1 - \pi\delta)}{\mu(1 - \pi\delta) + \hat{D}(1 - \mu(1 - \pi\delta))} \\
 &= \mu + \frac{(1 - \hat{D})(1 - \pi\delta)\sigma_{\eta}^2}{\hat{D} + (1 - \hat{D})(1 - \pi\delta)\mu}
 \end{aligned}$$

Proof of Proposition 1

Taking the total derivative of equation 2.12 with respect to both \hat{D} and π , we find:

$$\frac{\partial \hat{D}}{\partial \pi} \left[(1 - \mu(1 - \pi\delta)) \frac{C}{\gamma \sigma_{\epsilon} t a^2} \right] = -\delta \left[\frac{1}{[1 - \mu(1 - \pi\delta)]^2} - \mu(1 - \hat{D}) \frac{C}{\gamma \sigma_{\epsilon} t a^2} \right]$$

Next, we know that the probability of observing a layoff is $(1 - \hat{D})(1 - \eta(1 - \pi\delta))$, or $(1 - \hat{D})(1 - \mu(1 - \pi\delta))$ in expectation. Taking the total derivative with respect to π yields:

$$\frac{\partial \Pr[\text{Layoffs}]}{\partial \pi} = -\frac{\partial \hat{D}}{\partial \pi} (1 - \mu(1 - \pi\delta)) + (1 - \hat{D})\mu\delta$$

Combining the two results above and simplifying, we find:

$$\frac{\partial \Pr[\text{Layoffs}]}{\partial \pi} = \frac{\delta \gamma \sigma_{\eta}^2}{C [1 - \mu(1 - \pi\delta)]^2} > 0$$

Proof of Proposition 2

We can rewrite $\Delta \Pr[\text{Layoffs}] / \Delta (\text{Large Firm Layoff})$ as

$$\frac{\Delta \Pr [\text{Layoffs}]}{\Delta (\text{Large Firm Layoff})} = \frac{\Delta \Pr [\text{Layoffs}]}{\Delta (\pi)} \frac{\Delta \pi}{\Delta (\text{Large Firm Layoff})}$$

From Proposition 1 we know $\frac{\Delta \Pr [\text{Layoffs}]}{\Delta (\pi)} > 0$. Also from equation (2.13) we know that $\Delta \pi / \Delta (\text{Large Firm Layoff}) > 0$. These two inequalities imply

$$\frac{\Delta \Pr [\text{Layoffs}]}{\Delta (\text{Large Firm Layoff})} > 0.$$

Proof of Corollaries 3-5

Following from appendix A.5, we can write the following derivative as:

$$\partial \left[\frac{\Delta \Pr [\text{Layoffs}]}{\Delta (\text{Macro Event})} \right] / \partial \left[\frac{\Delta \pi}{\Delta (\text{Macro Event})} \right] = \frac{\Delta \Pr [\text{Layoffs}]}{\Delta (\pi)}$$

From appendix 4, we know the right hand side is positive. Therefore,

$$\partial \left[\frac{\Delta \Pr [\text{Layoffs}]}{\Delta (\text{Macro Event})} \right] / \partial \left[\frac{\Delta \pi}{\Delta (\text{Macro Event})} \right] > 0$$

Similarly from appendix 4, we know that

$$\frac{\partial}{\partial \sigma_{\eta}^2} \left[\frac{\Delta \Pr [\text{Layoffs}]}{\Delta (\pi)} \right] > 0$$

and that:

$$\frac{\partial}{\partial \gamma} \left[\frac{\Delta \Pr [\text{Layoffs}]}{\Delta (\pi)} \right] > 0$$

Therefore we get the following two results:

$$\begin{aligned} \frac{\partial}{\partial \sigma_{\eta}^2} \left[\frac{\Delta \Pr [\text{Layoffs}]}{\Delta (\text{Large Firm Layoff})} \right] &> 0 \\ \frac{\partial}{\partial \gamma} \left[\frac{\Delta \Pr [\text{Layoffs}]}{\Delta (\text{Large Firm Layoff})} \right] &> 0 \end{aligned}$$

Dynamic Version of the Model with Exogenous Updating Rule

In this appendix we present a simple dynamic extension of our model based on McCall's (1970) model of intertemporal job search. At date 0 there is a continuum of managers who evaluate their investment opportunities and hire a worker to engage in production. Thereafter in each period t , the project has a positive chance of failure, which depends positively on both the manager's quality given by η_i , and the time-varying aggregate state, given by $\lambda_{s,t}$. The states can be normal (N) or adverse (A), such that $\lambda_{N,t} = 1$, and $\lambda_{A,t} = \delta$. After observing the realization of the project on each date, each firm has to decide whether to continue or terminate production. Given the parameters of this model, it will always be optimal for firms to continue if their projects do not fail. Therefore, the key decision has to be made after the project fails.

The main cost of announcing a layoff is that the market's belief about the manager type is adversely affected when they observe a layoff. This is what we call *reputation cost*. In each period nature draws a reputation cost $z(\pi)$ for the manager. For simplicity, we take this reputation cost to be exogenously given. The market's belief that the aggregate is in a normal state at time t is given by π_t . The key assumption about the reputation cost is that $z'(\pi) > 0$, i.e. when the market thinks the aggregate state is more likely to be normal, then their posterior beliefs about manager talent will be more pessimistic after they observe a layoff. In each period there is a random draw of π , which maps 1-to-1 into a reputation cost. Let the distribution of reputation cost be given by $F(Z) = \Pr[z \leq Z]$, with $F(0) = 0$, $F(B) = 1$ for $B < \infty$. The manager has the option of not engaging in a layoff, in which case he pays c in this period and waits until next period for another draw of reputation cost from F . The per-period cost c is the net loss the firm bears every period by keeping the worker at a failed project for an additional period.

Let y_t be the manager's payoff in period t . To be consistent with the pre-existing search models it is convenient to characterize the payoff as $y_t = -\gamma z(\pi)$ if the manager with a failed project decides to layoff when the reputation cost is $z(\pi)$, and $y_t = -c$ when then manager decides to delay layoff. Here γ again measures the degree of reputational concern a manager has. The managers devise a strategy to maximize $E \sum_{t=0}^{\infty} \beta^t y_t$, such that $0 < \beta < 1$ is the discount factor.

Let $v(z(\pi))$ be the expected value of $\sum_{t=0}^{\infty} \beta^t y_t$ for an optimally-behaving manager who faces a reputation cost of $z(\pi)$, and is deciding whether to layoff or not. In this model we assume no recall. The value function $v(z(\pi))$ satisfies the Bellman equation

$$v(z(\pi)) = \max \left\{ -\frac{\gamma z(\pi)}{1-\beta}, -c + \beta \int v(z(\pi')) dF(\pi') \right\}$$

There exists a threshold reputation $z(\bar{\pi})$, such that if the manager is facing a reputation cost $z(\pi) \leq z(\bar{\pi})$, he should layoff, and delay otherwise. Solving for the threshold reputation, we can characterize his strategy as

$$\gamma z(\bar{\pi}) - c = \frac{\beta\gamma}{1-\beta} \int_0^{\bar{\pi}} [z(\pi) - z(\bar{\pi})] dF(\pi')$$

Further rewriting and by applying integration by parts we can characterize the reputation cost threshold as

$$\gamma z(\bar{\pi}) - c = \beta(\gamma E[z(\pi)] - c) + \beta\gamma \int_{\bar{\pi}}^{\infty} F(\pi') d\pi'$$

Thus, in this economy, managers whose project fails, will delay announcing layoffs until he faces a sufficiently high market-wide belief of being in an adverse aggregate state (i.e. a low value of π). This effectively generates periods of no or little layoffs, and large number of layoffs when market's belief about the aggregate state is adverse with high likelihood. In this model the firms need not all layoff in the same period. Their decision rule will depend on their degree of reputational concerns, γ . For a large class of functional forms for $z(\cdot)$ it can be shown that the threshold $\bar{\pi}$ is a decreasing function of γ . This suggests that when managerial reputational concerns rises (i.e. high γ), their threshold for waiting becomes more restrictive (i.e. lower $\bar{\pi}$), as these managers are waiting to engage in layoffs in periods when the market's belief puts a very high probability on the aggregate state being adverse. As an example consider the case in which F is a uniform distribution with support $[0, B]$. Additionally assume that $z(x) = \phi x$. Under this example, we can characterize the threshold as

$$\bar{\pi} = \frac{\beta \{B + \phi E[\pi]\}}{\phi + \beta} + \frac{(1-\beta)c}{\gamma(\phi + \beta)}$$

It is clear that $\partial \bar{\pi} / \partial \gamma < 0$, $\partial \bar{\pi} / \partial c > 0$, and $\partial \bar{\pi} / \partial B > 0$, while the effect of ϕ is ambiguous. This suggests that a greater reputational concern makes the threshold more

restrictive, while a larger cost c (which is a per-period loss made by the firm because of following the inefficient policy) and a higher variance of beliefs as measured by B , leads to a less restrictive threshold.

Appendix Tables

Table A1: Fisher's Method for Combining Results from Independent Scan Statistics

This table reports our results for Fisher's Method, which is a method to combine results from the independent Scan Statistics we compute for each non-overlapping interval of 20 or 60 business-days between 1970 and 2010. The top half of the table conducts this analysis for all layoffs in our sample, while the bottom half restricts the sample to layoffs in the 3 digit NAICS industries. The null hypothesis is to assume that there is uniform layoff propensity for each 20 or 60 day window (column (1)). The subwindow column (2) lists the length of window under consideration for excess clustering. The test statistic (column (4)) is a combined p-value of all individual tests, and is distributed with a chi-squared distribution. The degrees of freedom (column (5)) is simply twice the number of individual tests, which is given in column (3)). The last column lists the 'combined p-value' from Fisher's method. If this p-value is below 0.05 then we can reject the null of no excess clustering for the given subwindow.

Window (days)	Sub-window (days)	# of months	Test Statistic (Fisher's Method)	Degrees of Freedom	p-value (Fisher's Method)
All Firms					
20	1	325	507.99	650	1.000
20	3	325	626.82	650	0.736
20	5	325	708.67	650	0.055
20	10	325	781.41	650	0.0003
60	1	109	202.26	218	0.771
60	3	109	255.70	218	0.041
60	5	109	254.52	218	0.045
60	10	109	268.86	218	0.011
60	15	109	288.03	218	0.001
3-digit Industry Firms					
20	1	252	374.27	504	1.000
20	3	252	437.20	504	0.986
20	5	252	551.12	504	0.072
20	10	252	631.32	504	0.0001
60	1	83	181.26	166	0.198
60	3	83	192.81	166	0.076
60	5	83	198.53	166	0.043
60	10	83	213.82	166	0.007
60	15	83	212.48	166	0.009

Table A2: Public - Private Comparison over the Business Cycle (Matched on Size and 4-digit industry level)

This table exploits within-firm or within-industry variation to analyze differences in actual layoff behavior over the business cycle between public and private firms. The unit of observation is a the firm level tracked monthly between April 1995 and December 2010. In this table, a layoff constitutes a mass layoff as defined by the Bureau of Labor Statistics under its Mass Layoff Statistics Program (see section 4.2 for further discussion). In this table we report two sets of regressions, each with two different dependent variables. The first dependent variable is layoff indicator, which takes a value of one if the firm engages in a mass layoff in a given month. The second dependent variable is the number of workers laid off as a share of previous year's employees. The sample includes matched public-private pairs (see section 5.1.2 for the methodology) based on size (revenue) and 4-digit NAICS industry. These specifications rely on comparing each public firm to its matched private counterpart since we include matched-pair fixed effects. All regressions include controls for previous year's log revenue and its interaction with the recession dummy, and previous year's number of employees and its interaction with the recession dummy (except when the dependent variable is share laid off). Since the month fixed effects absorb all variation over time, the effect of a recession indicator is not separately identified in the specifications that include month fixed effects. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Sample	Matched Sample		Matched Sample	
	Layoff Indicator (1)	Share Laid Off (2)	Layoff Indicator (3)	Share Laid Off (4)
Public Indicator	-0.0207* (0.0121)	-0.0146*** (0.0046)	-0.0194** (0.0090)	-0.0117*** (0.0041)
Recession Indicator	0.0244 (0.0161)	-0.0006 (0.0021)		
Public x Recession	0.0301 (0.0203)	0.0029 (0.0025)	0.0312* (0.0185)	0.0115** (0.0048)
Mean	0.0569	0.0155	0.0569	0.0155
Std. Dev	0.2318	0.0267	0.2318	0.0267
Quadratic Time Trend & seasonality controls	✓	✓		
Month Fixed Effects			✓	✓
Matched-Pair Fixed Effects	✓	✓	✓	✓
Log Employees and Recession Interaction	✓		✓	
Log Revenue and Recession Interaction	✓	✓	✓	✓
Observations	10920	656	10920	656

Table A3: Follower Characteristics, 1970-2010 (Robustness Tests)

This table has the same results as in Table 8, but with firm fixed effects. In this table we report our results about the characteristics of public firms that layoff before and after the largest 20 firms in the economy as measured by previous year's revenue. Correspondingly the sample is restricted to the Fortune 500 constituents that are not the largest 20 firms as measured by revenue. The unit of observation is at the firm level observed over every business day between 1970 and 2010. There are two indicator variables that serve as dependent variables in the analysis. The follower indicator takes a value of one if in a given year the firm announces the layoff in a 5-day window following a large firm layoff (including the day of the large layoff). Similarly, the counterfactual follower indicator takes a value of one if in a given year the firm announces a layoff in a 5-day window prior to a large layoff (excluding the day of the large layoff). The first pair of regressions ((1) and (2)) are estimated using the entire sample (i.e. all Fortune 500 firms not in the top 20). The second pair of regressions ((3) and (4)) are estimated using only the sample of firms that laid off in a given year. By restricting the sample to this pool of firms, we are effectively relying on variation within firms that have relatively high propensity to layoff. In the last specification (5), we restrict the sample even further and include only firms that laid off within 5 days before or after a large firm announcement. Since in this case the two dependent variables are perfectly negatively correlated, we just need to estimate one regressions (instead of the pair). All the specifications include firm fixed effects, year fixed effects and firm level controls. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dependent Variable	Follower Indicator (1)	Counterfactual Follower Indicator (2)	Follower Indicator (3)	Counterfactual Follower Indicator (4)	Follower Indicator (5)
Sample	All Firms	All Firms	All Firms that Laid Off in Given Year	All Firms that Laid Off in Given Year	All Firms that Laid off within 5 days before/after large firm layoff
optionshare	0.1296*** (0.0356)	0.0117 (0.0282)	1.2822*** (0.3119)	-0.7038** (0.2738)	4.1088*** (0.4515)
TotalComp	-0.0013*** (0.0004)	0.0009 (0.0014)	0.0006 (0.0047)	0.0105** (0.0046)	-0.0343* (0.0179)
Ceo Tenure = 0 - 4 years	0.0178 (0.0122)	-0.0062 (0.0091)	0.3022* (0.1832)	-0.1993 (0.1538)	0.9573*** (0.2286)
Mean	0.0292	0.01763	0.2764	0.1645	0.6238
Std. Dev	0.1685	0.1316	0.4482	0.3715	0.4869
Firm Fixed Effects	✓	✓	✓	✓	
Year Fixed Effects	✓	✓	✓	✓	✓
Firm Level Controls	✓	✓	✓	✓	✓
Observations	2151	2151	228	228	101

Table A4: Compassionate CEOs and Layoff Announcements over the Business Cycle, 1970-2010

This table analyzes the impact of compassionate CEOs on layoff announcement propensity over the business cycle. The unit of observation is at the firm level tracked yearly between 1970 and 2010. The sample includes all contemporaneous constituents of Fortune 500 that ever announced a layoff. The dependent variable is an indicator variable, which takes a value of one when a firm announces a layoff in a given year, and zero otherwise. Our first measure of compassionate CEO is an indicator variable which takes a value of one, if the tenure of a given CEO at the firm before he was appointed CEO is greater than 5 years. This is used in specification (1) and (2). For specification (3) and (4) we change the cutoff from 5 years to greater than 15 years. In specifications (1) and (3) all firms in the sample are used for estimation, whereas in (2) and (4) we restrict the sample to firms that had no externally-appointed CEOs in the given year (i.e. only "home-grown CEOs"). All the specifications include year-fixed effects, firm-fixed effects, and firm-level controls and the interaction of each of these controls with the recession indicator. The firm level controls include Earnings-Price ratio, Book-to-Market Ratio, Market Capitalization, Leverage Ratio, Years since IPO, which are all obtained from COMPUSTAT. Note that since we include year-fixed effects, the recession indicator cannot be separately identified in these regressions. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dependent Variable	Layoff Indicator (1)	Layoff Indicator (2)	Layoff Indicator (3)	Layoff Indicator (4)
Sample	All Firms	Only Firms with "home-grown" CEOs	All Firms	Only Firms with "home-grown" CEOs
Recession x Years at Firm before CEO > 5 years	-0.0160 (0.0372)	0.0058 (0.0435)		
Years at Firm before CEO > 5 years	-0.0011 (0.0554)	-0.0194 (0.0588)		
Recession x Years at Firm before CEO > 15 years			-0.0129 (0.0413)	0.0048 (0.0437)
Years at Firm before CEO > 15 years			-0.1005* (0.0530)	-0.1240** (0.0573)
Mean	0.1438	0.1438	0.1438	0.1438
Standard Deviation	.3509	.3509	.3509	.3509
Firm Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Firm Level Controls and Recession Interactions	✓	✓	✓	✓
Observations	2295	1694	2295	1694

Appendix to Chapter 3

Proof of Propositions 3.1 - 3.3

Proposition 1: If stage i is performed in academia, then $\Pi_i = \alpha p \Pi_{i+1} - w_a$. If it is performed in the private sector, then $\Pi_i = p \Pi_{i+1} - w_p$. Because wages are non-negative, and α and p are both probabilities (and therefore less than one), it follows that in both cases, $\Pi_i < \Pi_{i+1}$. Thus, if a research stage is unconstrained either in academia ($\mu \alpha p \Pi_{i+1} > w_a$) or in the private sector ($\mu p \Pi_{i+1} > w_p$), then subsequent stages will also be unconstrained.

Proposition 2: From ADS, the condition for research stage i to be optimally performed in academia is $p \Pi_{i+1} < z$. The condition for the private sector to be unconstrained is $\mu p \Pi_{i+1} > w_p$. Combining these and using the fact that $\mu < 1$, we have: $\mu p \Pi_{i+1} > R + (1 - \alpha)z > R + (1 - \alpha)p \Pi_{i+1} > R + \mu(1 - \alpha)p \Pi_{i+1}$. This can be re-arranged as: $\mu \alpha p \Pi_{i+1} > R$, which is the condition for academia to be unconstrained in stage i .

Proposition 3: Suppose that a research line is just barely constrained in the private sector, so that $\mu p \Pi_{i+1} = w_p = R + (1 - \alpha)z$. For academia to be unconstrained, we need $\mu \alpha p \Pi_{i+1} > w_a = R$. Substituting for $\mu p \Pi_{i+1}$, we find the condition to be: $\alpha [R + (1 - \alpha)z] > R$, which simplifies to $\alpha(1 - \alpha)z > (1 - \alpha)R$, or $\alpha z > R$, as desired.

Proof of Proposition 3.4

Expanding the conditions for the transition point we find the following. In the unconstrained equilibrium, the transition point is the first stage to satisfy:

$$p \Pi_{i+1} = p \left[p^{k-i} V - (R + (1 - \alpha)z) \sum_{j=1}^{k-i} p^{j-1} \right] > z$$

In the constrained equilibrium, the transition point is the first stage to satisfy:

$$\mu p \Pi_{i+1} = \mu p \left[p^{k-i} V - (R + (1 - \alpha)z) \sum_{j=1}^{k-i} p^{j-1} \right] > R + (1 - \alpha)z$$

As shown in Appendix A.1., Π_{i+1} is increasing in i . Thus, in both cases, the LHS of the above expressions is increasing in i . Because equilibrium is based on equality or near-equality in the above expressions, the equilibrium transition point will shift to offset

changes caused by other variables. Thus, the transition to the private sector will occur earlier if other variables increase the LHS or decrease the RHS of the above expressions.

By inspection, we find the following. (i) Higher values of V lead to an increase in the LHS. (ii) Lower values of z lead to both an increase in the LHS and a decrease in the RHS. (iii) Lower values of R always lead to an increase in the LHS, and lead to a decrease in the RHS in the constrained equilibrium. (iv) Higher values of μ lead to an increase in the LHS, but only under the constrained equilibrium.

Thus, in the unconstrained and unconstrained equilibria, we know that i^* and \hat{i} , respectively, must fall and push the transition point earlier in response the the changes listed above.

Proof of Proposition 3.5

Let stage k be the first stage which occurs in the private sector in the absence of constraints. Without loss of generality, suppose that stage k is the final stage of the research line. Under the conditions of Assumption 1, if the funding state is high, the last stage occurs in the private sector and the payoff is $E(\pi_k^p)$. By contrast, if the funding state is low, the stage occurs in academia and the payoff is $E(\pi_k^a)$. In the preceding stage, if the funding state is high, the payoff is:

$$\alpha p \left[\frac{1+\rho}{2} E(\pi_k^p) + \frac{1-\rho}{2} E(\pi_k^a) \right] - w_a$$

By contrast, if the funding state is low, the payoff is:

$$\alpha p \left[\frac{1-\rho}{2} E(\pi_k^p) + \frac{1+\rho}{2} E(\pi_k^a) \right] - w_a$$

The value of $\Delta\Pi_{k-1}$ is defined as the difference between these payoffs. By direct calculation, we find:

$$\Delta\Pi_{k-1} \equiv \alpha p \left[\frac{2\rho}{2} E(\pi_k^p) - \frac{2\rho}{2} E(\pi_k^a) \right]$$

This simplifies to:

$$\Delta\Pi_{k-1} \equiv \rho\alpha p [\mathbf{E}(\pi_k^p) - \mathbf{E}(\pi_k^a)] = (\rho\alpha p)\Delta\Pi_k$$

By recursion, this relationship extends back to stages farther from the point of transition, leading to the desired relationship:

$$\Delta\Pi_{k-i} = (\rho\alpha p)^i \Delta\Pi_k$$

Proof of Proposition 3.6

As in the previous section, suppose that the value of performing the last stage in the private sector is $\mathbf{E}(\pi_k^p)$, but let the academic payoff be $\mathbf{E}(\pi_k^a) = 0$. While this is not a simple normalization, it serves to highlight situations where most of the value of a research line is based on having access to funding at the point of commercialization. Under assumption 1, the funding state is a sufficient statistic for the environment in which the last stage is performed. Thus, for the short research line, which is i stages from transition, the payoff in the high (low) funding state is:

$$\alpha^i p^i \left[\frac{1 \pm \rho^i}{2} \mathbf{E}(\pi_k^p) \right] - w_a(1 + \alpha p + \dots + \alpha^i p^i)$$

The long research line is $i + 1$ stages from transition, and its payoff is:

$$\alpha^{i+1} p^{i+1} \left[\frac{1 \pm \rho^{i+1}}{2} \mathbf{E}(\pi_k^p) \right] - w_a(1 + \alpha p + \dots + \alpha^{i+1} p^{i+1})$$

Comparing these payoffs, it follows that the short line is preferred if and only if:

$$\frac{1 \pm \rho^i}{2} \mathbf{E}(\pi_k^p) > \alpha p \frac{1 \pm \rho^{i+1}}{2} \mathbf{E}(\pi_k^p) - \alpha p w_a$$

If academic wages w_a are small relative to the payoff of the project in the final stage, then this simplifies further to:

$$(1 \pm \rho^i) > \alpha p (1 \pm \rho^{i+1}) \rightarrow 1 - \alpha p > \mp \rho^i (1 - \rho \alpha p)$$

In the above expression, the \pm and \mp signs take their upper operator in the high-funding state and their lower operator in the low-funding state. By inspection, in the high-funding

state, the short line is always preferred to the long line. Rearranging slightly, preference reversal occurs in the low funding state if and only if:

$$\rho^i \alpha p (1 - \rho \alpha p) > \alpha p (1 - \alpha p)$$

If the above inequality is satisfied, the long research line is preferred in the low-funding state, while the short research line is preferred in the high-funding state. This expression has the functional form of $x(1 - x)$, and leads to equality when $\rho = 1$. Because ρ is a measure of correlation, this is the greatest value it can take on. For the above inequality to be satisfied, the left-hand-side must increase as ρ decreases, meaning that its partial derivative with respect to ρ , evaluated at $\rho = 1$, must be negative. By direct differentiation, this derivative is $i(1 - \alpha p) - \alpha p$, leading to the first desired condition:

$$\alpha p > \frac{i}{i+1}$$

Under the above condition, preference reversal occurs for values of ρ which are just below one. This preference reversal will continue, and increase in strength, up to the point where the derivative of the left-hand-side with respect to ρ equals zero. Differentiating as before, the maximizing level occurs when $i(1 - \rho \alpha p) - \rho \alpha p = 0$, or when $\rho = \frac{i}{\alpha p(i+1)}$. Importantly, the second derivative with respect to ρ at this point equals negative one, and the third derivative equals negative two, or zero for the special case of $i = 1$. Because the third derivative is weakly negative, the function falls from its maximum more quickly as ρ increases, compared to when it decreases. Thus, a convenient shortcut for defining the range of values of ρ which satisfy the inequality is to use the maximizing level $\rho = \frac{i}{\alpha p(i+1)}$ as the mid-point, and use the limit value of $\rho = 1$ as the upper limit. While this range does not cover the full set of values which lead to preference reversal for $i > 1$, it offers a simple expression for the acceptable range, as desired.

$$\rho \in \left(\frac{2i}{\alpha p(i+1)} - 1, 1 \right)$$

In the special case of $i = 1$, because the third derivative of the left-hand-side is zero, the function is symmetric about its maximum, and thus the mid-point approach yields the exact range of values which satisfy the inequality necessary for preference reversal.

Appendix Tables

TABLE A1: FIRST-STAGE ROBUSTNESS TESTS					
	A1-1	A1-2	A1-3	A1-4	A1-5
	Dep. Var. = Log(Local Life Science VC Investment)				
Specification:	Low-VC Only	N. of IPOs	Fitted IPOs	IT IPOs	Low-Tech IPOs
Lag: Past 2 Years					
Log(Non-Life-Science IPOs)	0.105** (0.046)				
Log(# of Non-Life-Science IPOs)		0.218** (0.103)			
Log(Historical IPO Share * National IPOs)			0.130** (0.056)		
Log(Information-Technology IPOs)				0.093* (0.053)	
Log(Non-High-Technology IPOs)					0.055 (0.033)
Log(Academic Rank)	-0.308 (0.448)	-0.318 (0.351)	0.175 (0.361)	-0.292 (0.374)	-0.328 (0.377)
Log(MSA Income)	3.163 (2.666)	3.253 (1.969)	1.961 (2.030)	3.245 (1.983)	3.660* (1.970)
Time Controls	FE	FE	FE	FE	FE
MSA Controls	FE	FE	FE	FE	FE
Level of Observation	MSA-Year	MSA-Year	MSA-Year	MSA-Year	MSA-Year
N. of Observations	595	770	616	770	770
N. of MSAs	17	22	22	22	22

Standard errors clustered by Region in parentheses. Significance: * 10% ** 5% *** 1%

TABLE A2: PATENT CITATIONS BY HORIZON				
	A2-1	A2-2	A2-3	A2-4
Specification:	OLS	IV	OLS	IV
Dep. Var.	Log(Short-Term Citations)		Log(Long-Term Citations)	
Academia Indicator	0.121** (0.053)	0.119** (0.053)	0.095 (0.060)	0.093 (0.060)
Past 2 years:				
Log(Local VC Investment)	-0.011 (0.018)		-0.007 (0.019)	
Academia X Log(VC Investment)	-0.035* (0.020)		-0.067*** (0.024)	
Instrumented Log(VC Investment)		-0.037 (0.043)		0.024 (0.067)
Academia X Instrumented Log(VC)		-0.025 (0.025)		-0.067** (0.028)
Inventor Controls	FE	FE	FE	FE
Time Controls	FE	FE	FE	FE
School Controls	FE	FE	FE	FE
Level of Observation	Inventor-Year	Inventor-Year	Inventor-Year	Inventor-Year
N. of Observations	8250	8250	8250	8250
N. of Inventors	3208	3208	3208	3208

Standard errors clustered by Inventor in parentheses. Significance: * 10% ** 5% *** 1%